PUBLIC FINANCING OF RISKY EARLY-STAGE TECHNOLOGY

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PUBLIC FINANCING OF RISKY EARLY-STAGE TECHNOLOGY

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To my parents, aunt, and sisters,
and to Jem and our kids, Reine and Josh

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LIST OF SYMBOLS AND ABBREVIATIONS

AMD	Advanced Micro Devices
ATE	Average Treatment Effect
ATT	Average Treatment Effect on the Treated
CIA	Conditional Independence Assumption
CIS	Community Innovation Survey
CLRM	Classical Linear Regression Model
EU	European Union
GDP	Gross Domestic Product
KFS	Kauffman Firm Survey
MLE	Maximum Likelihood Estimation
NORC	National Opinion Research Center
NRH	Neyman-Rubin-Holland
OECD	Organization for Economic Cooperation and Development
OLS	Ordinary Least Squares
PSM	Propensity Score Matching
R&D	Research and Development
SBA	Small Business Administration
SBIR	Small Business Innovation Research Program
STTR	Small Business Technology Transfer Program
SUTVA	Stable Unit Treatment Value Assumption
USPTO	United States Patent and Trademark Office

SUMMARY

This dissertation examines the role of public investments in inducing small firms to develop risky, early-stage technologies. It contributes to expanding our understanding of the consequences of research, innovation, and entrepreneurship policies and programs by investigating in more depth the effect of the Small Business Innovation Research (SBIR) program on the innovation effort, ability to attract external capital, and other metrics of post-entry performance of small business start-ups using a new sample and estimation approach.

Unlike prior R&D subsidy studies that concentrated almost exclusively on European countries, this dissertation focused on small business start-ups in the United States using a new scientific survey of new firms. It integrated the Kauffman Firm Survey (KFS) from the Ewing Marion Kauffman Foundation with the SBIR recipient dataset from the U.S. Small Business Administration (SBA) and used advances in statistical matching to achieve better comparability between the treated and control groups of small business start-ups. The integrated KFS-SBA dataset, which contains both recipient and non-recipient small firms, and statistical matching allowed us to empirically construct the counterfactual outcomes of SBIR recipients.

This dissertation balanced the pre-treatment characteristics of SBIR recipients and non-recipients through propensity score matching (PSM). It constructed the comparison sample by identifying non-recipients with nearly identical propensity scores as those of SBIR recipients. Consistent with the propensity score theorem, observations with the same distribution of propensity scores have the same distribution of observable

characteristics. PSM made the comparison and treatment samples homogenous except in SBIR program exposure, making the fundamental assumption of ignorability of treatment assignment more plausible.

Using the realized outcomes of observationally similar non-recipient start-ups as the counterfactual outcomes of SBIR recipients, we found empirical evidence of the input additionality effect of the SBIR program. Had they not applied for and granted SBIR R&D subsidies, recipient start-ups would have spent only $185,000 in R&D, but with SBIR their R&D effort was significantly increased to $663,000, on average. The treatment effects analyses also found a significant positive effect of SBIR on innovation propensity and employment. However, it appears that public co-financing of commercial R&D has crowded-out privately financed R&D of small business start-ups in the United States. A dollar of SBIR subsidy decreased firm-financed R&D by about $0.16.

Contrary to prior SBIR studies, we did not find any significant “halo effect” or “certification effect” of receiving an SBIR award on attracting external capital. However, we discovered a different certification effect of the SBIR program: SBIR grantees are more likely to attract external patents. This finding also confirms that innovation requires a portfolio of internal and external knowledge assets as theorized by David Teece and his colleagues.

This dissertation’s empirical results may be relevant to the Small Business Administration, SBIR participating agencies, the U.S. Congress, other federal, state and local policymakers, small high-tech start-ups, and scholars in the field of science, technology, and innovation policy.

CHAPTER 1

INTRODUCTION

1.1. Background and Motivation

Innovation is the single most important determinant of long-run productivity growth and improved standards of living (Abramovitz, 1956; Baumol, 2010; Boskin & Lau, 1990; Lerner, 2009; Romer, 1986, 1990; Solow, 1956, 1957; Tassey, 1997). The belief in the value of innovation in economic growth is not mere "technological optimism" as Cohen and Noll (1991) put it, drawing at least from the economic success of the United States through the 21st century. The importance of technology-based growth, however, does not necessarily provide sufficient incentive for the private market to invest in innovation (Arrow, 1962; Lerner, 2009). Accordingly, the public sector has participated in the development of technological innovations by funding basic research[1], sponsoring technology research that supports agency missions[2] and mandates (e.g. national defense, health care, development of efficient energy sources), and increasingly, by providing R&D tax credits, encouraging cooperative research arrangements among firms, supporting technology transfer from academic to industrial laboratories, and cofinancing commercial R&D.[3] These policy interventions are intended to sustain technological change and progress. The strategic importance of continuous technology

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[1] The assumption here is that the pipeline or linear model of innovation (Bush, 1945) is true, i.e., results of academic research drive the production of new products and processes in the commercial sector.
[2] It assumes a spin-off model of innovation, that is, military technologies and other agency mission-oriented technologies diffuse to the private sector that will, in turn, develop commercial applications.
[3] This is not an exhaustive list of research, technology, and innovation policy tools.

development and innovation is further highlighted by global competition. For decades, other economies like Japan and South Korea have invested significantly in human and physical capital and developed the capability to innovate (not just imitate) in current technologies, leading to what Tassey (2007) called "convergence in national technological capacity". Continuing U.S. technological superiority cannot be assumed. It is critical that its national innovation system continuously provide mechanisms to encourage the steady production of new technology assets as a foundation for future economic growth.

Small and new enterprises have contributed significantly to the national innovation effort. In the 20th century, half of the most important inventions and innovations in the U.S. originated from small businesses or independent inventors (Wetzel, 1982 as cited by Van Osnabrugge & Robinson, 2000).[4] In a more recent study, Breitzman and Hicks (2008) found that small businesses are more productive in generating patents than their larger counterparts. A more nuanced conclusion is provided by Acs and Audretsch (1990), who showed that small firms contribute to innovation more than large firms in a number of industries, such as electronics and computing equipment, process control instruments, synthetic rubber, fluid meters and counting devices, engineering and scientific instruments, and measuring and controlling devices.[5] Small firms are more innovative than their larger counterparts in specific

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[4] This finding is hardly novel. As early as 1958, Jewkes and his colleagues documented that major innovations were developed outside of large firms.

[5] Large firms, on the other hand, are more innovative in tires and inner tubes, agricultural chemicals, general industrial machinery, food products machinery, ammunition, paper industries machinery, primary metal products, among others (Acs & Audretsch, 1990).

industries leading to a “division of labor” between small and large firms in innovation. This “division of labor” was validated by recent studies; for example, Almeda and Kogut (1997) showed that small business start-ups innovate in less crowded technological fields while large firms contribute in established or mature fields. Another important contribution of small firms and start-ups is job creation. Small firms create more new jobs than they eliminate. Birch (1979, 1981), using Duns & Bradstreet (D&B) data, first documented that small firms created most new jobs. While Birch’s study was not without critics, his major finding was substantiated and verified by subsequent studies. Especially during economic recessions, small and young firms are a net generator of jobs, unlike large and established firms, which cut more jobs than they create (Armington, Robb & Acs, 1999; Small Business Administration, 2009).

The Small Business Innovation Research (SBIR) program is a U.S. federal policy intervention that co-finances technology development with small enterprises, i.e., firms with less than 500 employees. This dissertation examines the effectiveness of the SBIR in inducing innovation effort among small business start-ups using a new sample and methods motivated by the counterfactual approach to causation.

1.2. The Small Business Innovation Research Program

SBIR is a well-established U.S. federal program. The U.S. Congress established SBIR through the Small Business Act of 1982. It is a government R&D subsidy program to small firms. By lowering the cost of R&D, SBIR can encourage small firms to undertake R&D more intensively.

The four goals of the SBIR are (1) to stimulate technological innovation, (2) to use small businesses to meet federal research and development needs, (3) to encourage participation by minority and disadvantaged persons in technological innovation, and (4) to increase private sector commercialization of innovations derived from federal research and development (P.L. 97-219).

SBIR is the largest federal R&D program for small and medium-sized enterprises, with funding exceeding $1 billion annually. Eleven federal agencies participate in the SBIR program: Department of Agriculture, Department of Commerce, Department of Defense, Department of Education, Department of Energy, Department of Health and Human Services, Department of Transportation, Environmental Protection Agency, National Aeronautics and Space Administration, National Science Foundation, and Department of Homeland Security. The Small Business Innovation Development Act as amended mandates these agencies to set aside 2.5 percent of their extramural R&D funds to implement SBIR.

The SBIR grants are awarded in two phases. Phase I R&D awards grants select small firms up to $150,000 for the short-term investigation of the scientific and technical merit and feasibility of a research idea. Phase II awards additional federal funds up to $750,000 each to develop Phase 1 research ideas that have strong commercial potential. Phase III, for which no SBIR funds are awarded, focuses on private commercialization of Phase II projects.

1.3. Research Questions

This dissertation addresses the following research questions:

1. What are the characteristics of small business start-ups that received SBIR program funds? Do recipient small business start-ups differ significantly from non-recipient start-ups?
2. What are the most important attributes of small business start-ups that contribute to successful SBIR application and selection?
3. Does the SBIR increase the R&D effort and innovation propensities of small business start-ups?
4. Does the SBIR expand the capacity of small business start-ups to attract external capital?
5. Does the SBIR have a positive effect on other metrics of post-entry performance of small business start-ups such as sales and employment size?

1.4. Potential Contribution to the Literature and Policy Relevance

This dissertation examines the role of public investments in inducing small firms to develop early-stage technologies. It improves upon previous studies that investigated the effect of SBIR by using a new sample and a new estimation approach. Most prior SBIR studies have only looked at recipient-firms. Relying on program recipients' report of their pay-off from participating in public programs may result in an upward bias in the estimation of the program effect (Storey, 2002). More importantly, samples that only contain recipient firms cannot test the program effect of public financing programs. Policy evaluation must always address the counterfactual outcome: what would have happened without the policy intervention? It thus requires observationally similar cases that did not receive the intervention or treatment.[6] Two SBIR studies (Lerner, 1999; Wallsten, 2000) used both recipient- and non-recipient firms, but constructed their sample by manually combining recipient and non-recipient firms. The use of rejected firms and firms that may not be eligible to participate in the SBIR program most likely did not lead to unit homogeneity between participating and non-participating small firms, which is a critical requirement for meaningful comparisons of mean outcomes between groups (Gelman & Hill, 2007). This dissertation can potentially fill up this void by building a new dataset of recipient and non-recipient small business start-ups.

Unlike prior evaluation studies, this study uses an inflow sample of small business start-ups. This inflow sample is a panel study of a cohort of firms that all started business operations in 2004. The main methodological advantage of an inflow sample is that it can rule out confounding effects of macroeconomic variables, as all businesses in the sample have been exposed to the same external factors. This inflow sample of start-ups is integrated with the SBIR recipient database to identify small high-tech business start-ups that received public financing from SBIR. As far as I know, this research is the first effort to integrate SBIR recipient data with a new sample of business start-ups. Thus, the resultant dataset is an important addition to the data infrastructure for research,

[6] Treatment is defined broadly in the methodology literature. It can be a drug or a new therapy administered to patients, a training program offered to displaced workers, or an educational innovation applied to a set of students.

innovation, and entrepreneurship policy studies.[7] Following Hall (2008) and Jaffe (1999), this dissertation attempts to simulate an experimental setting in order to construct the counterfactual outcomes of small business start-ups that received public financing. The study's empirical model follows the counterfactual approach to causation. Treating policy and program evaluation as a "missing data" problem, the model uses data from observationally similar non-recipient small business start-ups to impute the value of the unobserved counterfactual outcomes of new small high-tech firms that received public financing.

While this dissertation's estimation approach is applied to the evaluation of a federal technology program, it has wide applicability to other policy fields. Its emphasis on comparing comparable groups (achieved through statistical matching) and controlling for macroeconomic variables (by using an inflow or cohort sample) is relevant to the practice of policy and program evaluation, specifically on methods to improve the internal validity of treatment effect estimates.

Only a few evaluation studies have focused on the role of public financing on small business start-ups. The focus of this research is early-stage technology development by small business start-ups. From a Schumpeterian perspective, small business start-ups are agents of technical change because of their propensity to introduce new products and processes in emerging or less-crowded technological fields. These new technologies can potentially supersede current technologies and in the process, redefine

[7] Subject to the disclosure and confidentiality policies of the Kauffman Foundation and the National Opinion Research Center (NORC), the integrated dataset can be made available to other NORC researchers to further understand the production of new technologies by small business entrants.

new market opportunities than can sustain the innovating firm's and the nation's technological leadership and global competitiveness. The unique integrated dataset of small business start-ups allows the examination of the effect of public financing on risky early-stage development of technology.

In the 1980s, the U.S. government enacted a series of policy interventions to facilitate technological breakthroughs and innovations including the Bayh-Dole Act, the Stevenson-Wydler Act, the American Competitiveness Act, and the Small Business Development Act (which established the SBIR). The perception at that time was that the U.S. was losing its technological leadership and global competitiveness. These technology initiatives received bipartisan support. Increasingly, however, policymakers have demanded empirical evidence on the effectiveness and efficiency of these technology policy interventions. The main pressure point is the federal fiscal deficit that stood at 10 percent of gross domestic product (GDP) in 2009 and is projected to continue in the next decade (Congressional Budget Office, 2011). The higher the fiscal deficit becomes, the stronger the demand for cuts in public programs, which include support for research and innovation in small businesses. Two parallel movements in the public sector, evidence-based policy and performance management, are also gathering momentum (Cozzens & Melkers, 1997; Heinrich, 2007; Shapira & Kuhlmann, 2003). The demand to tie rigorous evidence and metrics (on which policy interventions work and do not work) with decisions about program design, funding and management highlights the importance of building and expanding our knowledge base on the performance of these technology policy initiatives. The termination of the Advanced Technology Program (ATP) in 2007 is testament to the greater scrutiny of public investments in commercial R&D. Although the SBIR has been reauthorized multiple times since its creation in 1982, the U.S. Congress has been lukewarm to recent initiatives to extend the program (Schacht, 2011).[8]

This dissertation furthers our understanding of the consequences of research, innovation, and entrepreneurship policies and programs. More fundamentally, it addresses whether a market failure exists in the production of early-stage technologies, that is, small high-tech business start-ups underinvest in productive capabilities to generate new technologies. An answer to this question is necessary to determine if technology policy interventions are matched with actual market failures and not with theoretically derived and assumed private underinvestment in R&D (Tassey, 2007). If firms do not underinvest in R&D, public resources are funding infra-marginal R&D projects (Wallsten, 2000), that is, R&D projects that would have been undertaken by small firms even without SBIR funding support. On the other hand, if firms underinvest in R&D, the economy is less likely to discover new technologies that may undergird its future economic growth and material prosperity. Firms that applied for but were not awarded with R&D funds either scaled back or abandoned the R&D project altogether, affecting innovation outputs and outcomes (Feldman & Kelley, 2003).

The treatment selection model identifies the characteristics of small business start-ups that contribute to successful SBIR application, selection, and participation, and thus expands our understanding of the characteristics of small high-tech start-ups that self-

[8] In 2011, SBIR was temporarily extended by Public Law 112-17. It was officially extended for another 6 years, ending September 30, 2017, after H.R. 1540 (The National Defense Authorization Act for Fiscal Year 2012) was approved and signed into law by President Obama in December 21, 2011.

select into SBIR and at the same time are adjudged potentially innovative by SBIR federal agencies. The results may also inform firm-level strategy. For example, it is possible that, *ceteris paribus*, start-ups that are located in knowledge networks like those found in California and Massachusetts are more likely to receive SBIR funding. It is also possible that all other relevant factors considered, start-ups that have produced patents are more likely to receive SBIR funding. The first result would suggest the importance of locating in knowledge-dense networks that facilitate innovations and the second result would suggest the importance of demonstrating absorptive capacity to increase the likelihood of participating in public financing programs. The development of productive capabilities (Teece, 1986; Winter, 2003) creates opportunities for the innovating firm to outcompete other firms in the industry. The innovating enterprise must use everything at its disposal, i.e., all resources available within its local knowledge network and the overall national innovation system to improve its productive capabilities. Public resources from federal agencies that support enterprise innovation are one of the available resources that can be tapped to strengthen the dynamic capabilities of firms. This dissertation contributes to our understanding of how SBIR resources may or may not strengthen the productive capabilities of small business start-ups.

1.5. Organization of the Study

Chapter Two reviews the theoretical and empirical literature on the effect of research and innovation policies and programs on firm outcomes and identifies gaps in the literature that the dissertation can potentially address. Chapter Three discusses the counterfactual approach to causation, specifically the assumptions needed to apply the approach to treatment effects analysis. Chapter Four discusses the implementation of matching and related estimators in causal analysis, the sample used in this study, and the empirical model of treatment selection and estimation. Chapter Five presents descriptive statistics and the results of the SBIR treatment selection model. Chapter Six discusses the empirical evidence on the treatment effect of the SBIR program on post-entry performance of small business start-ups. Chapter Seven provides the conclusions and theoretical and policy implications of the study as well as recommendations for future research.

CHAPTER 2

RELATED LITERATURE

This chapter reviews the theoretical and empirical literature on the effect of public programs that support research and innovation in the commercial sector. The first section presents the theoretical link between research, innovation, and technology policies that directly support private R&D and firm outcomes. The second section summarizes the key findings of studies that examine the impact of these policies and programs. The final section summarizes how this dissertation research extends or advances prior research.

2.1. Theoretical Link Between Public R&D Support Programs and Firm Outcomes

The theoretical support for public R&D programs originates from two research streams in economics: (1) the market failure argument derived from mainstream general equilibrium theory, and (2) the systems failure argument from the emerging evolutionary economic theory of technical change. The following discussion centers on market and systems failure in the generation of new technology by small firms and new enterprises.

2.1.1. Market Failure in the Production of Commercially Useful Knowledge

The more often used economic rationale for the public financing of commercial R&D is the market failure argument (Feldman & Kogler, 2008; Steinmuller, 2010) derived through formal economic modeling (Hall, 2008) that dates back to Arrow's (1962) seminal article on the economics of inventive ideas.

The knowledge required by firms to produce innovation is not like any other economic commodity. Although it can be traded or exchanged like conventional economic goods, the economic incentive to produce commercially useful knowledge is significantly weaker because innovators cannot realize a reasonable rate of return from their innovative activities (Geroski, 1995). This is called the problem of appropriability, which results from the three generic sources of market failure: (1) indivisibilities, (2) public goods and externalities, and (3) uncertainties (Arrow, 1962; Dasgupta & David, 1987; Geroski, 1995; Hall, 2008).

2.1.1.1. Indivisibilities

Undertaking R&D needs large fixed costs to set up the required technical manpower, facilities, and equipment. Firms may have to commit at least a "critical minimum level of innovation effort" (Metcalfe, 1995, p. 424) before R&D programs are expected to produce desired innovation outputs. The minimum scale requirement for R&D to be productive is higher for firms competing in the high-technology sector than their counterparts in the traditional sectors. For example, R&D projects in the high-technology sector may require specialized equipment or facility or a specific set of competencies from the R&D team,[9] which becomes part of the fixed cost of R&D. The minimum level of R&D effort results in indivisibilities, i.e., R&D facilities, equipment, and highly technical manpower can only be used efficiently when they are used at full capacity. Alternatively stated, the production of innovation is characterized by

[9] For example, research in nanotechnologies is increasingly multidisciplinary.

economies of scale (Cohen, 2001): the average cost of producing new product prototypes or production techniques or both declines as the firm engages in more R&D. The presence of large fixed cost and economies of scale implies that large firms may be more efficient than small firms in conducting R&D and in introducing innovations into the economy. The idea that large establishments are the most powerful drivers of technological progress can be credited back to Schumpeter (1942) and, more recently, to Galbraith (1967) and Lucas (1978). There are reasons why generating innovations is more efficient in large enterprises, that is, why average cost declines as R&D effort intensifies in large enterprises. Large firms possess complementary assets (e.g. large marketing and legal departments) that facilitate the production and protection of innovation (Teece, 1986; Winter, 2003). Large and established firms can also take advantage of experience and cumulative learning to screen out technological dead-ends allowing them to focus on more promising and feasible R&D projects. Small firms and start-ups, on the other hand, have limited financial and human resources to support R&D (Acs, 1999) and cannot enjoy economies of scale at lower levels of R&D effort. In sum, the minimum size of R&D teams and indivisible R&D facilities (Metcalfe, 1995) discourage firms, especially small business start-ups, from producing more innovation.

2.1.1.2. Public Goods and Positive Spillover Effects

Knowledge derived from either academic research or industrial R&D has properties of a public good, that is, it is both non-rival and partially non-excludable.[10]

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[10] A pure public good is both non-rival and non-excludable.

Knowledge is non-rival because it is not diminished by extensive use. For example, the technique to produce *Intel* microchips or the process to compress *Seagate*'s hard disks remains effective regardless of the number of times it is used in production, even by *Intel*'s and *Seagate*'s competitors. This is akin to the consumption of national defense, a traditional example used in textbook exposition of a public good: when residents of the state of Georgia consume or enjoy the benefits of a strong U.S. national defense system, it does not mean that the residents of other states enjoy less national defense. Most economic goods like personal computers, food, automobiles are rival goods.[11] If knowledge is non-rival then the marginal cost of an additional user is technically zero, but more realistically, close to zero, because the transmission of knowledge is not costless. As such, the private market will not provide knowledge resources efficiently when the price is set close to its marginal cost. Secondly, knowledge is partially non-excludable, that is, it is difficult to exclude others from using it. The standard solution to non-excludability is the establishment of property rights, of which patents and copyrights are prime examples. Without secure property rights, a *Silicon Valley* start-up, for example, may not be able to preclude other firms from reverse-engineering and copying its new product or production technique. In the absence of a patent regime, competitors will just wait for innovating firms and copy their innovative ideas, discouraging firms from investing in R&D. In short, non-excludability (or even partial non-excludability) encourages free riders to an innovation, weakening the general incentive to produce commercially useful knowledge.

[11] If there is only one *Dell* desktop PC in a *Best Buy* store and John bought it, it means that Jane will not be able to buy and enjoy the benefits of using the PC.

Closely related to non-excludability is the positive spillover effect of innovative ideas. The knowledge produced by a firm may be useful not only to other present or future R&D projects of the firm but also to other firms as well as scientists and engineers in universities and public R&D laboratories. For example, advances in microchip technology by *Intel* will benefit its competitors as well as manufacturers of personal computers like *Dell*, *Acer*, and *Apple*. A new commercial technology may also advance fundamental understanding of how the physical world works, and thus help extend the frontiers of scientific knowledge. Thus, from a larger societal point of view, a new product or process benefits not only the innovating firm but also other actors in the national economy and even the larger global economy. Due to this positive spillover effect, the marginal social benefit of the innovation is larger than the marginal private benefit that accrues to the innovator. Because the innovating firm only considers its marginal private benefit, it produces potentially commercially useful knowledge below the socially optimal level.[12]

The strength of the U.S. patent system may be insufficient to encourage small firm innovation. Cooper (2003) argued that small and new enterprises lack both (1) legal resources to protect their innovation from imitation, and (2) the market power to extract monopoly rents from their newly introduced innovation. Moreover, the patent system provides protection in only a very limited number of sectors (Geroski, 1995). The

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[12] Another example of an economic good with positive externality effects is basic education. The whole economy/society benefits when its citizens/residents consume more basic education as the latter is associated with more responsible citizenship and greater productivity. But because the individual only considers the benefit that he will enjoy with each additional year of basic education, and not the additional benefit that the whole society will enjoy, he is likely to consume less basic education than what the whole economy desires.

calculation of a lower marginal private benefit (due to non-excludability and spillover effects) by small firms and start-ups further lowers their innovation output away from the socially optimal level of innovation. Instead of performing more R&D, small enterprises may just rely on knowledge spillovers from academic research to generate innovative outputs (Feldman, 1994).[13]

2.1.1.3. Risks and Uncertainties

The risks and uncertainties associated with the process of generating innovation involve both (1) the outputs of R&D and (2) the financing of R&D activities. The two are inextricably linked because the second proceeds from the first. Innovative activities are difficult to finance because their output is highly uncertain.

The difficulty in financing innovation projects arises from the (1) technical, (2) market, and (3) competitive uncertainties in the production of innovation. First, the output of R&D is not a monotonic function of R&D inputs. Without dynamic capabilities and complementary assets (Nelson, 1996; Teece, 1986), firms cannot easily translate more R&D inputs into more innovation outputs. The creative process involved in innovation has a random element; business experimentation involves a lot of trial-and-error and the desired outputs and outcomes may not be achieved on the first few attempts. Second, the product prototype may not work on a commercial scale; for example, it may not be amenable to mass production, limiting market potential. In addition, the newly

[13] This assumes that academic research is linearly connected with product and process innovations at the firm level. It also presupposes sufficient absorptive capacity of small and new firms to capture the economic benefits of academic research.

introduced product may not enjoy market demand at a volume sufficient to recoup the cost of R&D. As Bhide (2008) has argued convincingly, firm innovation also involves significant risk-taking on the part of consumers; for example, personal computers and mobile phones (and now, smart phones) may not have been successful commercially without consumers taking risks that these new high-tech products have important personal economic uses. Third, the possibility remains that a competing firm develops a similar or more superior product or technology, limiting the returns to R&D or, worse, driving the firm out of the market. These uncertainties drive private enterprises to become risk-averse, discriminating against high-risk but potentially high-return R&D projects. Risk-averse businesses tend to produce only incremental innovation, not radical, cutting-edge, new-to-the-world innovation. Worse, they may opt to place their bets on the status quo, ignoring, for example, new production methods that have been tried, tested, and used by other firms. These strategic decisions that are influenced by a negative risk evaluation affect long-run productive capabilities for innovation.

The technical, market, and competitive uncertainties of engaging in R&D and innovation are more pronounced in small firms and start-ups than in large and established firms. Business start-ups may not possess sufficient absorptive capacity to develop the intended products and processes and the experience and network to take these innovations successfully to the market. More fundamentally, they may lack market research capability to establish demand before developing the intended new product or technology.

The second aspect is the uncertainty that results from the information asymmetry between entrepreneurs and providers of capital including banks and external investors (e.g. venture capitalists and angel investors), leading to a higher cost of external capital (Hubbard, 1998). Financiers may be reluctant to extend credit in the absence of a credible market signal of the quality and prospect of the firm's innovation project. When the innovation project is perceived as risky, capital providers usually require an extra premium to extend credit, pushing up the cost of external capital. Toole and Turvey (2009) have argued that information asymmetries in the financing of innovation are more problematic for small R&D performing firms. This problem is compounded when the stage of technology development cycle is factored in. Cooper (2003) found that small businesses do not have sufficient funding at the early stage of R&D, implying that capital providers are risk-averse in extending credit to innovation projects that are not "near-market" (Lerner, 1999; Shane, 2004). This leads to a substantial "financing gap" (Branscomb & Auerswald, 2003) that deprives firms that are willing to assume a portion of the risks the resources to develop early-stage technologies.

When external capital is difficult to secure, firms may rely only on internal capital. But internal capital is also limited, especially to small business start-ups. When external capital from financiers is prohibitively high and internal capital from the entrepreneurs themselves is limited, economically viable R&D projects may not be undertaken, generating a social welfare loss.

2.1.2. Systems Failure

The evolutionary economic theory of economic change and the systems theory of innovation (Dahlman & Nelson, 1995; Lundvall, 1992; Metcalfe, 1995; Nelson, 1993; Nelson & Winter, 1982) provide a much broader justification for technology and innovation policy, arguing that firms, especially small firms, have to conduct research and development (R&D) on their own and experiment on their own, in order to strengthen their absorptive capacities (Cohen & Levinthal, 1990), necessary to understand the current technological frontier, and access relevant technologies externally. These theories do not consider uncertainties, information spillovers, and the public good nature of knowledge as market failures. They argue instead that these so-called market failures are fundamental features of the market system (Metcalfe, 2007, 1995), and as such, they do not justify the adoption and implementation of technology policies to correct these market "imperfections."

Evolutionary theory perceives the generation and diffusion of innovation as a systems problem. Economically useful knowledge is not produced and disseminated when the system of innovation fails. Metcalfe (2007) identified at least two ways in which the innovation system may fail: (1) knowledge actors are missing, and (2) connections among producers of knowledge, among users of knowledge, and between producers and users of knowledge are absent. Thus, a firm may fail to produce innovation if its (1) absorptive capacity and (2) connection with knowledge producers and users in the innovation system are missing or not functioning. One of the means to increase absorptive capacity and connect with other system innovation actors is to conduct R&D. Systems theory suggests policy interventions to shoulder private sector risks in performing innovative activities to raise the experimental behavior of firms (Metcalfe, 2007), expand their absorptive capacity, and induce them to network with other users and producers of innovation as well as capital providers.

2.1.3. SBIR as a Solution to the Appropriability Problem and System Failure

The SBIR is an R&D subsidy to small firms, specifically to small high-tech firms. It can be construed as a government venture capital initiative where public financing underwrite the research and development of early-stage technologies and processes (Branscomb & Keller, 1998; Lerner, 1999). As Borrus and Stowsky (1999) put it, technology programs like the SBIR, which stimulate the development of new industrial technologies, could be regarded as bets on the country's technological future.

The SBIR subsidy helps small firms satisfy the required minimum scale of R&D (e.g. minimum size and competence of the R&D team) necessary to achieve results. It allows, for example, the hiring of university scientists or engineers to spearhead or support its R&D effort. More importantly, the SBIR grant may also enable the recipient small firm to engage in more R&D projects and thus utilize whatever R&D facility and equipment it has set up originally to full capacity. Engaging in more R&D projects while utilizing the same level of resources the firm possessed prior to the SBIR grant decreases the unit cost of innovation to be derived from the new and more intensified R&D effort. The recipient's innovative effort becomes less costly at a larger scale of R&D, which is exactly the competitive advantage of large firms with large R&D departments over small firms with meager R&D budgets.

Public financing shifts the recipient-firm's marginal cost to the right (David, Hall & Toole, 2000; Metcalfe, 1995) pushing its innovation effort theoretically up to a level that closes the gap between the private level and the "socially optimal level" of R&D. The economy-wide benefits of more small firms conducting R&D in the high-technology sector as a result of the availability of SBIR grants multiply when the knowledge derived from publicly co-financed R&D spills over to other users and producers of innovations. Public financing also alleviates the risks and uncertainties of the outcomes of R&D effort. By providing small firms the opportunity to engage in longer-term R&D, SBIR enables recipients to have a better estimate of the reward and risk of developing their intended new products or processes. Using the SBIR research grant, a better evaluation of the probability of technical and market success will encourage the recipient small firm to further develop the technology in the future with or without public financing. The prospect of having federal agencies procure the proposed technology to pursue their agency missions also lowers market uncertainties for some SBIR-financed research projects. The increase in the innovation effort of recipient firms as a result of public financing alleviating the problems of indivisibilities, technology spillovers, and negative risk evaluation is the so-called *additionality effect* of research, innovation, and technology policies (Clarysse, Wright, & Mustar, 2009; Georghiou, 2002).

The SBIR program offers financing to new innovative small enterprises to develop unproven but promising technologies (Toole & Czarnitzki, 2007). The availability of public financing to small high tech firms at the seed or start-up stage of technology development, a critical stage in which private investors like venture capitalists are still reluctant to participate (Cooper, 2003), encourages small enterprises to pursue technological innovations (Gonzalez, Jaumandreu & Pazo, 2005).

The SBIR grant can also have a *halo* or *certification effect* specifically in the application for external capital (Lerner, 1999; Link & Scott, 2010). Recipient small firms can leverage their SBIR awards to signal the "viability of the project and the company" (Siegel, Wessner, Binks, & Lockett, 2003, p. 124). SBIR's certification effect is similar to the warranty effect in Akerlof's (1970) "lemons" market problem[14]; the SBIR funding certifies that the innovation project is not a "lemon" and thus is worthy of capital infusion or credit extension at a rate lower than what would be possible when the capital providers do not have a hint on the quality of the innovation project. Using a Net Present Value model, Toole and Turvey (2009, p. 45) have shown that when initial public investment (such as that provided by SBIR grants) are used "to support research necessary to reduce technical and market uncertainties," capital providers will be encouraged to undertake follow-on investment. Thus, SBIR funding helps address the information asymmetry problem by certifying that the proposed new technology is both (a) "scientifically sound" and (b) "commercially promising" (Feldman & Kogler, 2008, p. 442), providing the extra push to financiers to extend additional capital funding. Lerner (1999) showed that small firms that received SBIR grants are three times more likely than non-recipients to attract venture capital, a finding validated by a more recent study by Toole and Turvey (2009) who documented that SBIR Phase I grants have a positive effect on receiving follow-on external private investment.

From an evolutionary economic perspective and national innovation systems approach, which rejects the role of public policy to achieve an "optimal" state of the innovation system, R&D subsidy programs like the SBIR are meant to "influence the nature of the knowledge base of the firm" and to "increase absorptive capacity" (Soete, Verspagen & Weel, 2010, p. 1169). The innovation system fails when producers of innovation, which are generally firms, do not have the capacity to translate or at least

[14] The product warranty certifies that the product is not a lemon.

adapt existing innovations and research produced by other firms, universities, and other research institutes to products, processes, marketing strategies, or organizational forms that can increase their own productivity and competitive advantage. The SBIR grant enables small firms to experiment with new processes, technologies, and organizational forms providing a critical opportunity for firms to learn by doing. Not all firm innovations are derived entirely from R&D; they also originate among others from the production floor, interactions between technical and nontechnical personnel, and interactions with users and customers. SBIR funding can have long-run *learning effects* that enhance the efficiency of future R&D programs of the recipient firm (David, Hall & Toole, 2000).

This dissertation focuses on testing the additionality and certification effects of the SBIR program as well as its effects on other metrics of post-entry performance like sales and employment size.

2.2. Related Studies

2.2.1. R&D Subsidy Studies

Existing research on the public financing of enterprise innovation has focused on testing the input additionality of R&D subsidies, asking whether public support for commercial R&D stimulates or crowds-out private R&D spending. [For a review of R&D studies, see David, Hall, and Toole, 2000; and Klette, Møen, and Griliches, 2000.]

Research and innovation policy evaluation studies have concentrated almost exclusively on European countries, due in large part to data availability. For example, Aerts and Czarnitzi (2004) evaluated the impact of R&D programs in Belgium; Czarnitzi and Licht (2006) and Hussinger (2008) in Germany; Clausen (2009) in Norway; and Busom (2000) and Gonzalez and Pazo (2008) in Spain. Since 1993, the European Union (EU) has conducted Community Innovation Surveys (CIS), which gather information to measure the effect of public funding on firm innovation inputs, outputs, and outcomes.[15] The U.S. does not have a comparable national firm innovation survey.[16] The only US firm survey that comes close to the CIS, which uses the Oslo Manual to measure innovation input and outputs, is the Georgia Manufacturing Survey conducted by the Georgia Tech Enterprise Innovation Institute. A number of studies have also focused on other member-countries of the Organization for Economic Cooperation and Development (OECD) using national R&D and innovation surveys of firms: Ozcelik and Taymaz (2008) on Turkey, Hall and Maffioli (2008) on Chile, Berube and Mohnen (2009) on Canada, Koga (2005) on Japan, and Lee and Cin (2010) on Korea.

These evaluation studies used cross-section, pooled cross-section, or panel data in their empirical analysis of the additionality effect of R&D subsidy programs. In terms of methods, most of these R&D subsidy studies have recognized the endogeneity of R&D subsidy programs. R&D subsidies are endogenous primarily because they are not randomly provided to firms. Firms self-select into these public subsidy programs. Accordingly, firms that apply for public financing are systematically different from those

[15] The CIS includes questions on product and process innovation, innovation activity and expenditure on R&D, effects of innovation, innovation cooperation, and public funding of innovation. The CIS contains the necessary information to measure the effect of public funding on firm innovation inputs, outputs, and outcomes. The 4th CIS was carried out in 2005 in 27 EU member states and three European Free Trade Area and EU candidate countries (OECD, accessed August 2010).

[16] The National Science Foundation (NSF) has sponsored a new Business R&D and Innovation Survey (BRDIS) which measures new variables like worldwide R&D expenses, R&D employee headcount, R&D expenses, and share of R&D devoted to new business areas and new science or technology activities. The NSF has released preliminary results from this survey.

that did not seek public funding.[17] The second consideration is the selection guidelines and criteria of R&D-granting agencies; winning applicants are more likely to differ from losing applicants in a number of important ways. In short, program or treatment selection is correlated with both observable and unobservable firm characteristics. R&D studies have used rigorous statistical and econometric techniques like propensity score matching, instrumental variable estimation, and fixed effects panel data analysis to address the endogeneity issues that result from the nonrandom selection of firms into R&D support programs.

The key finding from these R&D studies is that subsidized firms would have invested significantly less in R&D without the subsidy. However, not all studies concluded the absence of crowding-out. For example, Busom (2000) found that complete crowding-out cannot be ruled out in 30 percent of its Spanish sample while Clausen (2009) provided evidence that "development" subsidies in Norway substituted for private R&D spending. A subset of these studies also looked at the effect of R&D grants on other firm outputs and outcomes but did not find conclusive results. For example, Czarnitzi and Licht (2006) found a significant effect of public subsidies on patenting application among firms in Eastern Germany while Aerts and Czarnitzi (2004) did not find any significant difference in patenting behavior between subsidized and non-subsidized firms in Belgium.

[17] In the same manner, individuals who self-select themselves into a public job retraining program are systematically different from those that did not opt to participate in the program.

2.2.2. SBIR Studies

This section reviews the data and statistical methods used by studies that specifically evaluated the SBIR.

The early evaluation of SBIR was provided by Lerner (1999), Wallsten (2000), Link and Scott (2000), Audretsch, Wiegand and Wiegand (2002), and Audretsch, Link and Scott (2002). Lerner (1999) is the first attempt to evaluate the long-term impact of the SBIR program, with observations spanning over 10 years from 1983 to 1997. Its sample included (a) 541 small firms that received Phase II awards and (b) a comparison sample that received only Phase I awards, small firms matched on firm size and industry classification, and another set of small firms matched on firm size and geographical location. Lerner showed that SBIR-supported firms grew significantly faster in both volume of employment and sales than non-recipient small firms of similar geographic location and industry classification. He also found that SBIR awards interacted with local venture capital activity, that is, recipients had better sales and employment outcomes only in areas with substantial venture capital activity. SBIR had no impact on firm outcomes in areas where few institutions that provide external capital funding operate.

Wallsten (2000), in contrast, is a short-term evaluation of SBIR in terms of its input additionality effect. Its sample included (a) 367 small firms that received at least one SBIR award for the period 1990-92 and (b) comparison sample of small firms composed of 90 rejected firms (i.e., firms that applied for SBIR funding but was not awarded) and 22 eligible firms that did not apply for SBIR funding. Using an instrumental variable approach, he showed that SBIR crowded-out firm-financed R&D, disconfirming the input additionality hypothesis.

Wallsten (2000) and Lerner (1999) artificially constructed their comparison samples. The treatment sample and the comparison sample were obtained from different distributions. Manually matching on industry classification and geographic location may not have been sufficient to remove the endogeneity bias that results from the nonrandom selection of SBIR awardees.

Audretsch, Wiegand and Wiegand (2002), Audretsch, Link and Scott (2002), and Link and Scott (2000) only used SBIR recipients in their empirical analysis. The first study used (a) case studies of 12 SBIR recipient firms and (b) a mail survey of another 20 SBIR recipient firms in Indiana to show that SBIR influenced the career paths of academic scientists and engineers to form new firms. The second study used tobit regression on 112 Department of Defense (DOD)-supported SBIR recipients to show that SBIR-supported firms commercialized new products and services developed through SBIR funding. Link and Scott (2000) also interviewed SBIR awardees for 44 projects and estimated that the social rate of return of SBIR funding was at least 84 percent, that is, SBIR projects were socially valuable.

The main disadvantage of the second set of studies is that the sample was restricted to SBIR-supported small firms. By design, these studies are reflexive studies or before-and-after studies; the small firms themselves were used as controls. The weakness of these reflexive studies is that they cannot rule out selection and endogeneity bias (Rossi, Lipsey, & Freeman, 2004). It was possible that SBIR-supported firms commercialized the products and processes they generated through SBIR because they spent more in R&D, had a more capable management team, were more networked, were located in regions where external financing was easier to secure, and a host of other firm-level and location-specific factors that positively impact firm performance. Comparing the performance of small firms before and after SBIR funding does not also rule out simultaneity; more innovative firms are more likely to receive SBIR funding and to be more innovative in the future. In short, in the presence of selection and simultaneity, studies that used only recipient firms are seldom internally valid; we cannot rule out the effect of other factors outside of the SBIR award. More rigorous alternative methods to the reflexive approach are methods motivated by the counterfactual approach to causation, which will be discussed in the next chapter. The counterfactual approach to causation compares the post-funding outcomes of SBIR-financed small firms with the post-funding outcomes of observationally similar or comparable group of small firms that did not receive R&D subsidy.

Lerner's study used non-statistical matching to construct a comparison sample for 541 SBIR-recipient firms. The idea was to balance the characteristics of the two groups (i.e., the recipient firms and the matched firms) before their outcomes were compared. However, the study used only two variables simultaneously (i.e., firm size and industry classification, and firm size and geographical location) to find non-SBIR firms that matched the SBIR-funded firms on these two attributes. It is clearly an attempt to construct the counterfactual outcome of SBIR-funded firms (instead of just comparing their pre- and post-funding outcomes as is practiced in before-and-after studies), but matching only on two variables may not be sufficient to control for endogeneity or selection bias. The SBIR-supported firms and non-supported firms may still be systematically different from each other even when firm size and geographical location (and firm size and industry classification) were controlled for at the baseline. For example, SBIR-financed firms may be older, have spent more R&D, have a higher ratio of scientific and technical personnel (as opposed to non-technical and administrative personnel), grown faster, and a host of other factors that confound the cause-and-effect relationship between SBIR funding and firm performance. Controlling these other factors in a regression framework may not be enough (Gellman and Hill, 2007; Ho, Imai, King & Stuart, 2007). Wallsten (2000) also constructed a comparison sample that included rejected firms and potentially eligible firms that did not apply for SBIR funding. The comparison sample may not have come from the same distribution as that of the treated sample. It is also possible that since the two groups of firms are not balanced in pre-treatment characteristics, the regression was forced to infer beyond the data. As we will see in the next chapter, differences in pre-treatment characteristics between recipient and non-recipient firms makes the empirical results dependent on functional forms (Ho, Imai, King & Stuart, 2007).

2.3. Summary Discussion

Most SBIR studies only used recipient or treated firms in the empirical analysis. The survey methodology literature (or even the economics literature on games and strategy) finds that asking program recipients to report the pay-off from participating in public programs (or any other programs for that matter in which they will potentially benefit in the future) may result in an upward bias in the estimation of the treatment effect parameter. Two SBIR studies, specifically Lerner (1999) and Wallsten (2000), used non-recipient firms and employed techniques like instrumental variable estimation and non-statistical matching to address the endogeneity of R&D subsidy. However, the two studies combined the recipient sample with non-recipient sample that are not identically and independently distributed. The use of rejected firms and firms that may not be eligible to participate in the SBIR program most likely did not lead to unit homogeneity between treated and untreated firms, which is a requirement for meaningful comparisons of mean outcomes between groups (Gelman & Hill, 2007).

Advances in the micro-econometrics of program evaluation can better handle endogeneity and support the requirement of achieving unit homogeneity between groups to allow more meaningful causal inferences. A key implication of Lerner (1999) and Wallsten (2000) is the need to use better data to establish causal connections between public R&D subsidy programs and firm innovation and productivity. Empirical analysis of program effects using both recipient and non-recipient firms that are part of the same random sample of firms (like in EU's CIS) is a significant improvement over synthetic samples that are manually combined as if they came from the same distribution. This dissertation merges the Kaufmann Firm Survey, which is a survey of firms founded in 2004 and subjected to follow-up surveys in 2006, 2007, 2008, and 2009, with federal program databases to identify surveyed firms that received public R&D funding from SBIR. The merged dataset is a unique inflow sample of small firms that either receive or did not receive SBIR funding from the federal government.

Thus, this dissertation contributes to the literature on research and innovation policies and programs by measuring the treatment effect of the SBIR program using a better sample (i.e., an inflow sample of firms from the same random sample) and more rigorous methods from the advances in the micro-econometrics of treatment, policy, and program evaluation.

CHAPTER 3

METHODOLOGY

3.1.The Neyman-Rubin-Holland Counterfactual Framework of Causal Analysis

This dissertation seeks to identify the causal impact of a federal research and innovation program on firm-level outcomes.

A widely-accepted approach to causality is the counterfactual approach, which can be traced back to Lewis (1973). Dissatisfied with the regularity approach to causality that requires universal conjunction of events to identify causes and effects, Lewis (1973) redefined "X has caused Y" as "Y would not have occurred if it were not for X" (Pearl, 2009; Guo & Fraser, 2010). Thus, the counterfactual approach to causality considers what would happen if X did not occur. Before causality can be attributed from X to Y, Lewis (1973) requires the following two conditions (1) Y increased as a result of X, and (2) Y did not increase because X is not present. In short, the presence of X should produce a net effect in Y and its absence a zero net effect. To establish that X has caused Y, it is not enough to demonstrate that X and Y occurred together; the second condition, which is the counterfactual condition, must also be true. To establish that the second condition is true, Lewis's (1973) approach is to identify the "closest possible world," where X does not occur, and observe that Y does not also occur. To illustrate, following Brady (2008), if X is a government training program and Y is earnings, then the "closest possible world" can be defined as the world where the government training program does not occur but everything else (e.g. macroeconomic environment) is similar. Following Lewis (1973), (1) if in the factual world both government training (X) and increase in earnings (effect on Y) occurred, and at the same time, (2) if in the "counterfactual" world (where everything else is similar with the factual world) except for the absence of the government training program, earnings did not increase, then one can argue that the government training program (X) causes earnings (Y) to increase.

In statistics, the counterfactual approach to causation was further developed by Neyman (1923), Rubin (1974), and Holland (1986). Thus, the counterfactual framework is commonly known as the Neyman-Rubin-Holland (NRH) counterfactual framework of causal analysis or treatment effects analysis.

The NRH counterfactual framework assumes that every individual in the target population has two potential outcomes, i.e., (1) potential outcome with the treatment[18] and (2) potential outcome without the treatment (Cameron & Trivedi, 2005; Morgan & Winship, 2007; Woolridge, 2002). For this reason, the NRH counterfactual framework is also known as the potential outcomes framework. A causal effect or treatment effect is defined as the difference between the two potential outcomes. Following the standard formalization of potential outcomes, let Y_{i1} denote the potential outcome for unit *i* if the unit receives the treatment (or participates in a program) and Y_{i0} denote the potential outcome for the same unit if it does not receive the treatment (or does not participate in a program). Also, let T_i be a treatment indicator which is equal to 1 if unit *i* is treated and 0 otherwise. Thus, the individual causal or treatment effect (ITE) α_i can be defined formally as:

$$\text{ITE} = \alpha_i = Y_{i1} - Y_{i0} \qquad [1]$$

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[18] A treatment can be a drug or a new therapy administered to patients, a training program offered to displaced workers, or an educational innovation applied to a set of students.

The observed (or realized) outcome Y for individual *i* is:

$$Y_i = T_iY_{i1} + (1\text{-}T)Y_{i0} \quad [2]$$

$$Y_i = Y_{i1} \text{ if } T_i\text{=}1$$

$$Y_i = Y_{i0} \text{ if } T_i\text{=}0$$

Extending this definition for a single individual to a set of individuals, we may define the average treatment effect (ATE) as the difference between the mean potential outcomes, or more formally:

$$ATE = E(\alpha_i) = E(Y_{i1} \mid T_i\text{=}1) - E(Y_{i0} \mid T_i\text{=}0) \quad [3]$$

We can also define the average treatment effect on the treated (ATT). ATE and ATT represent closely related but different population parameters. ATE is the average causal effect of the treatment on randomly selected individuals in the target population. ATT, on the other hand, is the average causal effect for those that receive the treatment or participate in a program, i.e., those who have a high probability of receiving the treatment or program. ATT is equal to ATE conditional on T being equal to unity. Thus, ATT can be defined as:

$$\begin{aligned} ATT &= E(\alpha_i \mid T_i\text{=}1) \\ &= E(Y_{i1} \mid T_i\text{=}1\ ) - E(Y_{i0} \mid T_i\text{=}1) \end{aligned} \quad [4]$$

The so-called fundamental problem of causal inference (Holland, 1986) arises because only one of the potential outcomes is observable for each individual. We cannot observe both potential outcomes simultaneously. The counterfactual outcome, by definition, is not observable. This is the central challenge of program evaluation or treatment effects analysis. Let us consider both cases. First, if the individual participates in a program (or receives a treatment), the first term Y_{i1} of Equation 1 is observable but the second term Y_{i0} is not. Likewise, if the individual is not a program participant (and therefore, not in the treatment state), the second term is observable but the first term is not. Thus, using the NRH definition of causal connection, there is no direct way to identify the individual treatment effect for any particular case.

By extension, the average causal effect ATE or ATT cannot also be estimated because only one of the two average potential outcomes [i.e., either $E(Y_{i1})$ or $E(Y_{i0})$ and either $E(Y_{i1} \mid T=1)$ or $E(Y_{i0} \mid T=1]$ is observable. In short, the causal inference problem is a "missing data" problem. As a result, the researcher cannot directly compare the observed factual outcome and the unobserved counterfactual outcome in order to infer causal effect (Brady & Collier, 2004).

3.2.Solving the "Missing Data" Problem in Counterfactual Causal Analysis

Finding a solution to the "missing data" problem is akin to being able to identify Lewis's "closest possible world." Finding an empirical surrogate for Lewis's counterfactual world is finding a substitute for the counterfactual outcome of interest.

A set of assumptions has to be made to apply the NRH counterfactual framework in program evaluation or treatment effects analysis (Guo & Fraser, 2010; Sekhon, 2008). Without these assumptions, one cannot use observable outcomes as substitutes for unobservable counterfactual outcomes or make valid comparisons between observed outcomes of two groups.

The first assumption is the Stable Unit Treatment Value Assumption (SUTVA), which assumes that the potential outcomes of individuals is unchanged (hence, "stable") regardless of the changes in treatment status of other individuals (Morgan & Winship, 2007). That is, the treatment assignment for one individual does not influence the outcome of another (Gelman & Hill, 2007). According to Heckman (2005), SUTVA rules out the following two situations: (1) effect of treatment assignment patterns on potential outcomes, and (2) social interaction and general equilibrium effects. First, SUTVA is valid when potential outcomes do not vary with treatment assignment patterns. Morgan and Winship's (2007) example when SUTVA is violated is the situation in which potential outcomes change with the number of treated individuals, leading to the treatment becoming more or less effective as more individuals participate in the treatment. Second, SUTVA is valid if the gain/loss an individual obtains from the treatment/program does not spill over to other individuals. The assumption is thus violated when a treated individual interacts with an untreated individual benefitting the latter. Without the SUTVA, it would be difficult to uniquely define individual causal effect (Brady, 2008). For example, if treated individuals interacted with untreated individuals and spillover effects cannot be ruled out, then it would be difficult to isolate the treatment effect from the spillover effect. Lewis' "closest possible world" where everything else is the same except for the treatment is not achieved because the factual world and the empirical surrogate of the counterfactual world might also be the same in treatment status as a result of the spillover or interaction effect.

The second assumption is the conditional independence assumption (CIA) or ignorability of treatment assumption, which can be formally expressed as:

$$(Y_0, Y_1) \perp\!\!\!\perp T \mid X, \text{ or} \qquad [5.a]$$

$$(Y_0, Y_1) \perp T \mid X \qquad [5.b]$$

The CIA assumes that conditional on observed covariates X, the treatment and potential outcomes are independent. Put differently, treatment assignment or treatment status conveys no information about the values of the potential outcomes after observable characteristics X are held constant. It thus implies that participation in the program or treatment does not depend on expected outcomes, after controlling for the variations in outcomes dues to differences in X (Cameron & Trivedi, 2005). The CIA is also called "selection on observables" because the conditioning covariates are assumed to be known, observed, and measured without error (Barnow, Cain, & Goldberger, 1980). If the CIA holds then the treated individuals and untreated individuals will come to have the same mean values of Y_0 and Y_1 (Collier, Brady & Seawright, 2004). This solves the "missing data" problem, because if the CIA holds, we can swap the observed outcomes of untreated individuals for the unobserved counterfactual outcome of treated individuals and the observed outcomes of treated individuals with the unobserved counterfactual outcomes of untreated individuals. To summarize, causal inferences can only be made when both the SUTVA and the CIA are plausible.

3.3. Achieving Conditional Independence or Ignorability of Treatment Assignment

3.3.1 Experimental Studies

The most effective way of achieving conditional independence or ignorability of treatment assignment is through an experimental research design.

The key feature of the experimental design is randomization, i.e., experiments randomly assign individuals to either the treatment or control group.[19] Randomization ensures that the potential outcomes are independent of treatment. Knowing whether an individual receives treatment conveys no information whatsoever about his potential outcome under the treated state Y_{i1} or about his potential outcome under the control state Y_{i0}. If treatment is not randomly assigned, it is possible that individuals who think they will gain more from the treatment will self-select themselves into the treatment, and accordingly, knowing an individual's treatment status may convey information about his potential outcomes, and thus treatment and outcomes are not independent.

An experimental research design does not only satisfy the CIA [i.e., $(Y_0, Y_1) \perp\!\!\!\perp T \mid X$]; it satisfies a stronger unconditional independence assumption [i.e., $(Y_0, Y_1) \perp\!\!\!\perp T$], which is also called full independence assumption. Full independence implies conditional independence.[20] If (Y_0, Y_1) and T are independent, then the mean potential outcomes of the treated and untreated groups in an experiment are equal. More formally,

$$E(Y_{i1} \mid T_i=1) = E(Y_{i1} \mid T_i=0) = E(Y_i \mid T_i=1) \qquad [6.a]$$

$$E(Y_{i0} \mid T_i=1) = E(Y_{i0} \mid T_i=0) = E(Y_i \mid T_i=0) \qquad [6.b]$$

Equation 6.a states that the expected potential outcome of the treated when they are actually treated is equal to the expected potential outcome of the untreated had they been treated. If this is the case, then we can use the observed outcome of the treated as empirical substitute for the unobserved counterfactual outcome of the untreated had they

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[19] The classic experimental design also features measurement of the outcome variable before and after the intervention.
[20] If variables X and Y are independent, then $E(Y \mid X) = E(Y)$.

been treated. Equation 6.b states that the expected potential outcome of the untreated when not treated is equal to the expected potential outcome of the treated had they not been treated. The implication is that we can also substitute the observed outcome of the untreated for the unobserved counterfactual outcome of treated had they not been treated. Thus, in experimental studies, in which the research design ensures that the treatment assignment and the outcomes are independent, the group mean difference of the treated and untreated groups can identify the ATE defined earlier in Equation 3:

$$\begin{aligned} ATE &= E(Y_{i1} \mid T_i=1) - E(Y_{i0} \mid T_i=0) \\ &= E(Y_i \mid T_i=1) - E(Y_i \mid T_i=0) \qquad \text{by Eq. 6.a and 6.b} \end{aligned}$$

$$\begin{aligned} \text{ATE Estimator}_{EXPERIMENTS} &= E(Y_{TREATED}) - E(Y_{CONTROL}) \qquad [7] \\ &= E(Y_{i1}) - E(Y_{i0}) \end{aligned}$$

Experimental studies supply the correct unobserved counterfactual, thus solving the "missing data" problem in causal analysis. The group mean difference between the treatment and control groups [i.e., $E(Y_{i1}) - E(Y_{i0})$] provides consistent and valid estimates of the ATE.

3.3.2 Observational Studies

Experiments are usually not feasible in public policy research. For example, it is not normally politically acceptable to randomize participation to welfare and anti-poverty programs, labor training programs, support programs for businesses, or any other public programs for that matter.[21] Politics usually allow individuals of the target population to

[21] Randomization has been allowed in some education innovation programs.

self-select themselves into federal, state, or local public programs that they deem beneficial for themselves. Using randomization, the government, for example, may award welfare programs only to select qualified applicants while keeping other qualified applicants in the control group. This is neither ethically nor politically acceptable, especially in democratic polities. In the context of this dissertation, an experimental research design to estimate the causal effect of research and innovation policies and programs is also very difficult to implement politically because it may require, for example, not awarding R&D program funds to "winning" firms and/or awarding program funds to "losing" firms just to establish the net contribution of the program to important output and outcome measures. For all these reasons, policy researchers usually work with observational or non-experimental data in evaluating public policies and programs.

Rosenbaum (2002, vii) defines an observational study as "an empiric investigation of treatments, policies, or exposures and the effects they cause… [where] the investigator cannot control the assignment of treatments to subjects."

In observational studies, the investigator has no direct control on the allocation of individuals to treatment or non-treatment states (Cox & Reid, 2000) and accordingly, treatments are observed (hence, the term "observational") rather than assigned (Gelman & Hill, 2007; Brady & Collier, 2004). In short, random assignment is missing. The data from the KFS survey individually or in combination with other datasets that can identify firms that benefitted from federal S&T programs is an observational data.

Without random assignment, the CIA assumption does not hold in observational studies. Therefore, using the group mean difference between treated and untreated groups cannot identify the ATE. This study follows Morgan and Winship (2007) and calls the difference in observed outcomes of the treated and untreated groups (in observational studies) the naïve estimator. In this case, we substitute (1) the unobserved counterfactual outcome of the treated group [i.e., $E(Y_{i0} \mid T_i=0)$] with the observed outcome of the untreated group [i.e., $E(Y_i \mid T_i=0)$] and (2) the unobserved counterfactual outcome of the untreated group [i.e., $E(Y_{i1} \mid T_i=0)$] with the observed outcome of the treated group [i.e., $E(Y_i \mid T_i=1)$]. The naïve estimator does not consistently estimate ATE because (1) $E(Y_{i0} \mid T_i=0)$ and $E(Y_i \mid T_i=0)$ and (2) $E(Y_{i1} \mid T_i=0)$ and $E(Y_i \mid T_i=1)$ are rarely equivalent when the CIA assumption is not plausible, and thus the "missing data" problem is unsolved. The realized outcome of the treated group cannot substitute for the unobserved counterfactual outcome of untreated group, while the realized outcome of the untreated group cannot swap for the unobserved counterfactual outcome of the treated group as in Equations 6.a and 6.b in experimental studies when the independence assumption holds. The naïve estimator will include a bias, as shown by Angrist and Pischke (2009):

$$\begin{aligned} ATE_{NAIVE} &= E(Y_i \mid T_i=1) - E(Y_{i1} \mid T_i=0) \\ &= [E(Y_{i1} \mid T_i=1) - E(Y_{i0} \mid T_i=1)] + [E(Y_{i0} \mid T_i=1) - E(Y_{i0} \mid T_i=0)] \quad [8] \\ &= ATT + \text{Selection Bias} \end{aligned}$$

When the CIA does not hold, the naïve estimator will include a selection bias in its estimation of ATE. When the CIA holds, the last two terms [i.e., $E(Y_{i0} \mid T_i=1) - E(Y_{i0} \mid T_i=0)$] of the second line of Equation 8 becomes zero because they will both be equal to $E(Y_i \mid T_i=1)$ when potential outcomes are independent of the treatment assignment. See Equations [6.a] and [6.b].

The last two terms in Equation [8] is the difference in Y_{i0}s between those who are treated and those who are not. This is the difference in potential untreated outcome between the two groups. The selection bias (that results from the difference in Y_{i0}s of the two groups) can be either positive or negative. If the treated group has higher values of Y_{i0}s than the untreated group, the selection bias is positive, leading to an overestimation of the average causal effect. This happens, for example, when those who self-select (or are selected by program administrators) and eventually admitted to say, a public training program are those who are better educated than those who did not participate in the program. The potential outcomes of the better-educated treated group are expected to be higher than that of the untreated group. In this case, treatment assignment is not ignorable or is not independent of outcomes; the value of the treatment indicator can yield information on the value of the potential outcomes. A simple difference in group means will overestimate ATE. In the same vein, if the treated group has lower values of Y_{i0}s than the untreated group, the selection bias is negative, yielding an underestimation or even the elimination of the average causal effect altogether. Using the same example, a negative selection bias occurs when those who self-select into the job training program have lower levels of education than those who did not participate in the program.

Letting p equal the proportion of individuals who receives treatment, Morgan and Winship (2007) further decomposed the naïve estimator into ATE, differential treatment effect bias, and baseline bias as follows[22]:

$$ATE_{NAIVE} = E(Y_i \mid T_i=1) - E(Y_{i1} \mid T_i=0)$$

[22] It can be deduced from Morgan & Winship's (2007) and Angrist & Pischke (2009) decomposition of the naïve estimator that ATE = ATT + Differential Treatment Effect Between Treated and Untreated Cases.

$$= \text{ATE} + (1\text{-p})[\ E(\alpha_i \mid T_i{=}1) - E(\alpha_i \mid T_i{=}0)]$$

$$+ [E(Y_{i0} \mid T_i{=}1) - E(Y_{i0} \mid T_i{=}0)] \qquad [9]$$

$$= \text{ATE} + \text{Differential Treatment Effect Bias} + \text{Baseline Bias}$$

The differential treatment effect bias is equal to the difference of the treatment effect between those who self-select into the treatment and those who decided not to receive treatment. Using the same example above, this type of bias results when the outcomes of those who are treated increase more than the outcomes of those who are not treated. In short, the treated participants are more likely to benefit from the treatment than the untreated participants would if the latter get treated. The baseline bias is equivalent to the selection bias of Angrist and Pischke (2009) in Equation [8].

To solve selection bias in observational studies, one of the techniques is covariance control[23] either through (1) regression analysis or (2) matching. Angrist and Pischke (2009) believes that when the CIA and other assumptions of the CLRM[24] hold, the OLS estimate of the coefficient of a binary treatment variable in a regression of a set of observable attributes X and the treatment variable on an outcome variable can have a causal interpretation. The strict exogeneity assumption in CLRM is often expressed as $E(\varepsilon_i \mid X) = 0$, which also implies that (1) the unconditional mean of the error term is zero, and (2) the observed covariates X are orthogonal to the error term, that is, $E(x_j\varepsilon_i) = 0$ (Hayashi, 2000). Thus, strict exogeneity assumes the absence of (1) omitted variable bias, (2) simultaneity, (3) measurement error, (4) sample selection, and

[23] Other methods include (1) Heckman's sample selection model, (2) instrumental variable approach, and (3) regression discontinuity design.
[24] A model is set of restrictions on or assumptions about the joint distributions of the outcome and explanatory variables (Hayashi, 2000).

(5) contemporaneous dependence (Cameron & Trivedi, 2005; Greene, 2003; Woolridge, 2002; Hayashi, 2000), or in the context of Barnow, Cain, and Goldberger's (1980) "selection on observables," covariates X are known and observed. When the assumptions of the CLRM hold (which include strict exogeneity), the regression coefficient α in the following linear regression model is an unbiased estimate of the average treatment effect (Angrist & Pischke, 2009):

$$Y_i = \tau + \alpha T_i + X_i'\gamma + \varepsilon_i \qquad [10]$$

The estimate of the parameter α is obtained by minimizing the error sum of squares. Assuming X is a vector of explanatory variables including the treatment variable T, the OLS estimate of α is:

$$\hat{\alpha} = (X'X)^{T} X'Y \qquad [11]$$

The basic assumption that the outcome variable is linear in the treatment variable may not be plausible in causal analysis. The linearity assumption means that the treatment variable has a constant marginal effect on the expected values of the outcome variable Y. This is violated, for example, when the treatment effect is higher for those who receive the treatment than those who did not receive the treatment had they been treated. This is the case of differential treatment effect between treated and untreated cases as discussed above. In causal analysis, a heterogeneous treatment effect is a more plausible assumption than a constant or homogenous treatment effect. The linearity assumption of the CLRM is often violated in treatment effects analysis.

More fundamentally, the strict exogeneity assumption in the CLRM model with a treatment indicator as one of the predictor variables may be very difficult to defend. Heckman and Robb's (1989, 1986, 1985 as cited by Morgan and Winship, 2007) decomposition of the ATE coefficient in an OLS regression model showed that OLS will most likely have an omitted variable bias (and thus violate the strict exogeneity assumption and the CIA) because of the absence of the variable to measure individual's anticipation of the treatment effect, which theoretically is a significant predictor of outcomes. If this is the case, then the selection is on unobservables (and not on observables), yielding an omitted variable bias, or simply endogeneity bias. For this reason, the treatment variable is often modeled as endogenous in causal analysis, and not as an exogenous variable as in OLS models. Parametric estimators like the OLS are model dependent and thus, susceptible to bias when the model is misspecified (Ho, Imai, King, & Stuart, 2007).

Gelman and Hill (2007, p.199) have also shown that "causal inferences are cleanest if the units receiving treatment are comparable to those receiving the control." They believe that it is difficult to control for the confounding covariates through OLS regression when the distribution of the covariates differ across treatment status. Moreover, Lee (2005) has argued that regression analysis forces the researcher to compare incomparable units, and thus the statistical technique is incompatible with the effort to find the "closest possible world." This is the reason why Ho, Imai, King and Stuart (2007) have recommended balancing the data before performing regression analysis.

An alternative to regression analysis is matching, a method motivated directly by the counterfactual approach to causation. It aim is to reorganize the original sample (Gelman & Hill, 2007), or more specifically, to create a synthetic sample (Cameron & Trivedi, 2005) or a strategic subsample (Morgan & Winship, 2007) that includes a comparison group that is similar in observational attributes to the treated sample. All treated cases are retained and all unmatched untreated cases (i.e., untreated cases that are not observationally similar with any of the treated cases) are dropped from this synthetic sample. The sample of well-matched untreated units can serve as empirical surrogate for the control group that is constructed by randomization in experimental settings. In this sense, matching can be construed as an attempt to mimic the randomization process in experimental studies (Khandker, Koolwal, & Samad, 2010) or to create a “quasi-experimental contrast” (Morgan & Winship, 2007) by balancing observed covariates X across treatment status. To review, the consequences of randomization are (1) the independence of treatment assignment and outcomes, and (2) the balancing of observed covariates X and unobserved factors ε between treatment and control groups. When the two groups are balanced and under specific assumptions, the remaining differences in observed average outcomes between treated and untreated observations can be construed as the causal or treatment effect.

Once the matched comparison sample has been constructed out of the larger dataset, causal parameters can be identified by a sample difference in average outcomes of the two groups. An alternative technique is to use standard regression methods (e.g., linear regression, logistic regression) to estimate treatment effect in the area of overlap (Gelman & Hill, 2007; Ho, Imai, King, & Stuart, 2007).

Matching assumes, in addition to SUTVA and the CIA, that an overlap exists between the distributions of observed covariates X of the treated and untreated cases. More formally, the overlap assumption is expressed as:

$$0<\Pr(T=1 \mid X)<1 \qquad [12]$$

The overlap condition requires that there be untreated cases that have the same covariate distribution as the treated cases. If this assumption is violated, there will be treated observations that cannot find a good match among the untreated observations. The CIA and the overlap assumption constitute what Rosenbaum and Rubin (1983) calls the “strong ignorability of assignment” assumption, which is necessary for identifying the treatment effect.

Heckman et. al (1998) showed that in the estimation of ATT, the CIA can be relaxed to mean independence, that is, the untreated outcomes are the same across treatment states. More formally,

$$Y_{i0} \perp\!\!\!\perp T \mid X \quad [13]$$

which implies that,

$$E(Y_{i0} \mid T_i=1, X_i) = E(Y_{i0} \mid T_i=0, X_i) = E(Y_{i0} \mid X_i) \quad [14]$$

The ATT as defined in [4] can thus be estimated by:

$$\text{ATT Estimator}_{\text{MATCHING}} = E_{X_i \mid T=1}\,[E(Y_i \mid T_i=1, X_i) - E(Y_i \mid T_i=0, X_i) \quad [15]$$

The outer expectation in [15] is taken over the distribution of $X_i \mid T=1$, that is, the distribution of observed X in the treatment group. The overlap condition for identifying ATT requires that the support of X for the treated sample be a subset of the support of X for the untreated sample (Sekhon, 2008). This implies that untreated observations whose covariate values are outside of common support will be dropped in the estimation of ATT. Only treated cases and matched untreated cases are retained in the analysis. Dropping observations outside of common support will improve unit homogeneity between treated and untreated cases, making policy and program evaluation more meaningful (Guo & Fraser, 2010). In addition, Rosenbaum (2005) has shown that improving unit homogeneity[25] (a) not only reduces variability of the estimates of treatment effects, (b) but also their sensitivity to unobserved bias.

One of the advantages of matching estimators is that it is nonparametric. Unlike the OLS estimator, it avoids the assumption that the treatment effect enters the conditional mean function linearly (Cameron & Trivedi, 2005). More importantly, since matching excludes observations outside of common support, the analyst is not forced to compare incomparable units (Lee, 2005) making causal inferences more meaningful (Guo & Fraser, 2010). It is also a especially useful method if ATT is the parameter of interest. In policy analysis and evaluation, we are less concerned about the effect of the policy, program, or treatment on a randomly selected member of the population, which the ATE parameter represents. In most cases, the more relevant parameter is ATT, which the matching estimator (and its variants) can identify.

This dissertation uses matching estimators to construct the counterfactual outcomes of small business start-ups that received SBIR funding to measure the effect of public financing on the development of risky early-stage technology.

[25] Rubin (1974) and Holland's (1986) definition of unit homogeneity is sufficient to allow causal inference without the CIA (Collier, Seawright, & Munck, 2004).

CHAPTER 4

DATA AND MODELS

4.1. Implementing Propensity Score Matching and Related Estimators

This dissertation takes advantage of advances in statistical matching techniques to estimate the causal effect of SBIR on firm outcomes. It also combines matching with regression-based methods. Following Ho, Imai, King, and Stuart (2007), I also use standard regression models (e.g. linear and logistic regressions) to estimate the effect of SBIR after balancing the data. This is to ensure comparison between comparable groups (following the Neyman-Rubin-Holland counterfactual approach) when performing regression analysis.

The central idea of matching is to control for observable heterogeneity by finding in the untreated group "look-alikes" of treated participants. When implemented manually, matching is a tedious exercise. In practice, matching directly on observable attributes becomes more difficult the larger the set of covariates to match. This is called the "Curse of Attribute Dimensionality". For illustration purposes, let us assume that we are looking for a "look-alike" or a match of a small business start-up that has 10 employees, is located in California, is currently competing in the computer equipment industry, performs in-house R&D, recorded a profit in 2005, and is managed by its owner-founder who has a postgraduate degree. Finding a close match (much less an exact match) of this SBIR-recipient small firm is very difficult if not impossible. This dimensionality problem can be significantly reduced by matching on the propensity score, i.e., the conditional probability of treatment or program participation. Thus, instead of an empirical strategy of constructing a comparison group with identical covariates X, the alternative strategy, which this dissertation adopts, requires a comparison group that has a similar distribution of covariates X with that of the treatment group by matching on the propensity score. The propensity score is formally expressed as:

$$\text{Propensity Score} = P(T=1 \mid X) \quad [16]$$

Thus, propensity score matching (PSM), which originated from Rosenbaum and Rubin (1983), is a statistical method to match treated and untreated cases on the basis of the propensity score, which is a scalar variable, instead of manually matching on a vector of variables. If the strong ignorability of assignment assumption[26] holds, the use of the matched comparison group to construct the counterfactual outcome of treated cases is sufficient to remove selection bias, yielding a valid and consistent estimate of the mean impact of treatment (Heckman et. al, 1998; Rosenbaum and Rubin, 1983).

To summarize, the aim of matching is to balance the covariate distribution between the treated sample and the matched comparison sample. An important statistical result from Rosenbaum and Rubin (1983) is that it is enough to match on the conditional probability of treatment or the propensity score. On average, observations with the same distribution of propensity scores will have the same distribution of observed covariates X. Thus, matching on propensity scores, the ATT estimator in [14] can be reexpressed as:

$$\text{ATT Estimator}_{PSM} = E_{P(X_i \mid T=1)}\left[E(Y_i \mid T_i=1, X_i) - E(Y_i \mid T_i=0, X_i)\right] \quad [17]$$

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[26] Strong ignorability of assignment assumes both (1) presence of overlap and (2) mean independence. See Equations 12-15 on pages 46-47.

The PSM estimator of ATT in [17] implies that untreated observations whose propensity scores are outside the support of the propensity scores of the treated observations will be discarded.

To implement PSM, this dissertation followed the following matching protocol (Caliendo and Kopeinig, 2008, 2005) to construct the comparison group for treated firms. First, I divided small business start-ups into (1) those that receive SBIR financing (the treatment group) and (2) those that did not (potential control group). Second, I ran a logistic regression to model the participation of small business start-ups in the SBIR program and obtain estimates of their propensity scores. PSM predicted the probabilities of participation (propensity scores) of both treated and untreated small business start-ups using relevant covariates to be discussed in section 4.3. The propensity score model included variables that affect both treatment assignment and outcomes (Rosenbaum & Rubin, 1983; Gelman &Hill, 2007).

Third, I excluded from the sample non-recipient small business start-ups whose propensity scores are either (1) lower than the minimum propensity score of the recipient small firms or (2) higher than the maximum propensity score of the recipient firms to satisfy the key identifying assumption of the PSM estimator, which is the presence of a "common support" between the two groups.

Fourth, I paired each participant *i* with some group of comparable non-participants on the basis of the estimated propensity scores. I used the nearest neighbor matching algorithm i.e., search for non-participant *j* with the closest propensity score. I followed Abadie and Imbens (2002) who suggested using four matches for each treated participant.

Fifth, I assessed matching quality. The matching procedure should balance the distribution of the relevant independent variables in both the treatment and the comparison group. After the matching, the covariates should be balanced in both groups and hence no significant difference should be found. If there are significant differences, covariate balancing is not completely successful and remedial measures are necessary. For instance, Caliendo and Kopeinig (2005) recommended including high-order polynomial terms and/or cross-product interaction terms in the estimation of the propensity score to improve the match between the treatment and comparison groups.

Sixth, I computed the treatment effect as the difference between the mean outcome of the treatment group and the mean outcome of the comparison group. Specifically, the input additionality effect is the difference in the mean R&D expenditures of SBIR recipients and the mean R&D expenditures of observationally similar non-recipient small business start-ups and the certification effect is the difference in mean external financing. Estimating the treatment effect on other firm-level outcomes (e.g. employment and innovation propensities) followed the same approach. For statistical inference, the standard error of the treatment effect was estimated using Abadie and Imbens' (2006) bias-corrected variance estimator.

In the treatment effects analyses, the size of the comparison and treated subsamples varies from one model to another. PSM balances the covariate distribution of the recipient and non-recipient groups by dropping untreated observations that are not observationally similar to the treated cases. Recipient small firms that are off common support and those with missing values for a particular outcome variable will also be dropped from the treatment effects analyses.

Finally, following Ho, Imai, King, and Stuart (2007) and Gelman and Hill (2007), I also estimated the treatment effect by using regression-based methods after the observable characteristics of the treated and matched comparison subsamples. Regression analysis was only applied within the common support of X between the two groups. For example, a linear regression as in [10] can be estimated. In the case of a dichotomous outcome variable, the following logistic regression where α is the key parameter of interest can be fitted by maximum likelihood estimation.

$$\log (Y_i/1-Y_i) = \tau + \alpha T_i + Z_i'\gamma + \varepsilon_i \qquad [18]$$

In a regression framework, the estimate of the treatment effect is the coefficient (α) of the binary treatment variable T. The regression coefficient α is interpreted as the difference in mean outcomes between SBIR recipients and non-recipients, holding constant a set of confounding variables Z in the model. For statistical inference, the OLS variance estimate $V(\hat{\alpha})$ is:

$$V(\hat{\alpha}) = s^2 (X'X)^{-1} \qquad [19]$$

where the estimate of the error variance $(s^2) = SSE/(n-k) = e'e/(n-k)$.[27]

4.2. Data and Sample

The data for this study comes from the Kauffman Firm Survey (KFS). The Kauffman Foundation has granted me access to their confidential KFS micro-data through the data enclave managed by the National Opinion Research Center (NORC).

[27] SSE is the sum of squared error, n is the sample size, and k is the number of regressors including the constant.

The KFS is an inflow sample of 4,928 businesses founded in 2004 and tracked ever since. In an inflow sample, data collection is based on a random sample of all individuals (in this case, firms) entering the state of interest (in this case, entrepreneurship or operating a business) and followed until some pre-specified date, or until the spell ends (Jenkins, 2004). In short, the KFS is a longitudinal survey of the same cohort of firms that started operation in 2004. From a research design standpoint, this inflow sample or cohort sample structure ensures that the start-up firms faced the same external environment during their founding year and subsequent years of operation. For example, using an inflow sample we can dismiss the confounding effect of macroeconomic variables such as inflation rate, interest rate, or consumer confidence because all members of the inflow/cohort sample have been exposed to the same external factors since their founding in 2004. In contrast, using a standard population sample based on a general survey of firms, we might be comparing a small firm founded in 1980 and a second small firm founded in 1990. The economic environment in the 1980s is different from that of the 1980s. An inflow or cohort sampling design minimizes confounding factors such as the effect of a more favorable external environment on starting a business.

I also requested the Small Business Administration (SBA) to provide me a datasheet of SBIR and STTR recipients from 2004-08.[28] The start period of the SBIR recipient database coincides with the start of the KFS. To identify start-ups in the KFS sample that also received SBIR financing, I requested the Kauffman Foundation and the

[28] SBA provided the data through formal Freedom of Information Act (FOIA) request. I purchased the data from SBA for $400.

Mathematica Policy Research[29] to integrate the KFS and the SBIR recipient datasets. The data integration used the Data Universal Numbering System (DUNS) number of the sample firms as merging variable. The DUNS number is a unique numeric identifier assigned to a single business entity making it an ideal merging variable. The integrated dataset identified 25 small business start-ups that received SBIR financing to develop new technologies in 2007-08. In the empirical analysis, each of these 25 recipient small business start-ups is matched with at most four (4) observationally similar non-recipient small business start-ups.

I restricted the sample of potential controls to small firms. I dropped from the analysis all start-ups that have more than 500 employees prior to the 2007-08 treatment period.

4.3. Propensity Score Model

This dissertation estimates the effect of receiving SBIR financing on firm-level innovation inputs, outputs, and outcomes of small business start-ups. The primary estimator is the propensity score matching (PSM) estimator. As discussed in Section 4.1, the propensity score model includes covariates that affect both program participation and selection and firm-level outcomes (Rosenbaum & Rubin, 1983; Gelman & Hill, 2007).

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[29] Mathematica conducted the KFS survey for the Kauffman Foundation.

4.3.1. Post-entry Performance (Y): Inputs, Outputs, and Outcomes

Post-entry performance of small business start-ups is measured by several indicators. A common measure of innovation input is R&D expenditure. It can be measured either as an interval-level variable (in dollars) or a dichotomous variable coded 1 if the small business start-up engaged in R&D in 2008 and/or 2009 and 0 otherwise. Measures of innovation outputs include (a) patents and (b) product and process innovations. A patent is awarded to inventions that are both novel/non-obvious and useful. A product innovation is the introduction of a new or significantly improved good or service, whereas a process innovation is the implementation of a new or significantly improved process or method of providing services.

Another measure of post-entry performance is the ability of start-ups to attract external capital (e.g. loans, venture capital) necessary to run day-to-day operations, start production, or to expand the business. Traditional measures of performance like employment size were also used.

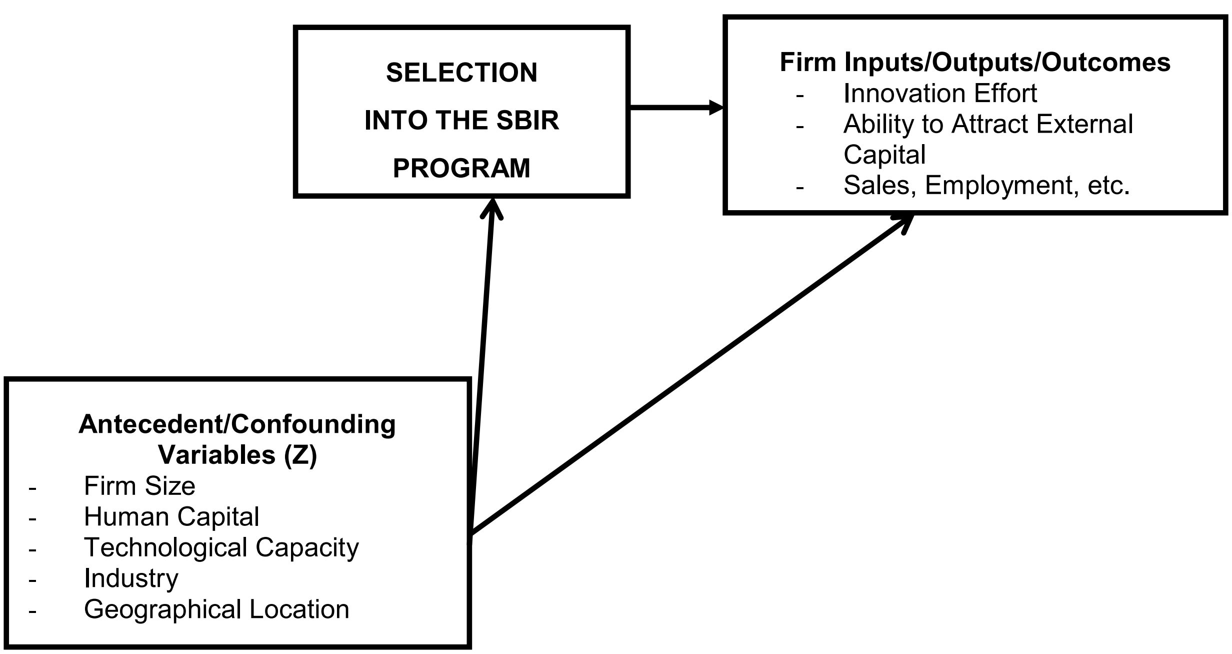

Figure 4.1 Propensity Score Model

4.3.2. Treatment Variable (T)

The treatment variable is a binary variable coded 1 if the small business start-up received SBIR/STTR funding in the 2007-08 period and 0 otherwise.

4.3.3. Covariates (X) that Affect Both Treatment Selection (T) and Post-entry Performance (Y)

I hypothesize that selection into the SBIR/STTR program as well as post-entry performance are affected by the start-up's size, human capital, technological capacity, industry, and location of operation.

4.3.3.1. <u>Firm Size</u>

Larger business start-ups have more resources to attract quality manpower, withstand random shocks in the external environment, and raise more capital for operation, production, and expansion. Larger firms are also more likely to possess specialized complementary assets (e.g. specialized channels of information) to successfully commercialize a new production technique or product prototype (Teece, 1986).

Firm size can also serve as proxy for the start-up's efficiency and ability to compete (Bruderl, Presisendorfer & Ziegler, 1992). Starting a new enterprise is inherently risky; thus, firms that have larger resource endowments in their initial years may be firms that are more confident about the efficiency of their production levels and more optimistic *ex-ante* about their probability of success in the market. If efficiency and ability to compete underlie the choice of a start-up's initial size, then external parties can use firm size to draw inferences about the quality of the firm. Unlike established small businesses, start-ups do not have a track record to speak of when applying for an SBIR grant. SBIR reviewers can use firm size as one of the filters to separate start-ups that have the potential to take advantage of the R&D grant from those that do not. Firm size is measured by the number of employees of the start-up at the start of its business operations in 2004.

4.3.3.2. Human Capital of the Entrepreneur

Human capital refers to the knowledge, skills, and experience that the founders or owners of the start-up possess. It increases the ability of the start-up to compete successfully. At a strategic level, the knowledge assets of the entrepreneur are critical in searching for and recognizing new business opportunities that are commercially promising (Shane, 2000). Research in entrepreneurship and creativity has shown that the breadth of one's training and experience strengthens the ability to relate two seemingly unrelated concepts to create something novel and useful. At the operational level, greater human capital of the founders increases firm productivity. Owners with more knowledge and experience are more efficient in organizing and more capable at attracting clients and external support (e.g. loans, research grants). More knowledgeable and experienced entrepreneurs also bring with them best-practice organizational routines that are important in running day-to-day operations and planning for the long-term (e.g. new markets to exploit and new products and processes to develop). In short, like firm size, the founders' human capital can proxy for the expected productivity or efficiency of the new enterprise. Banks, venture capitalists, and other capital providers as well as SBIR grant reviewers can use observable characteristics like the founder's human capital to infer about the quality of the business start-up. I measured human capital by the level of education and prior work experience of the start-up's founders.

4.3.3.3. Technological Capacity

Technological capacity refers to the ability of the start-up to generate potentially commercially useful research. This is typically measured by prior performance of R&D and patent production. Engaging in R&D is an important innovative activity because it increases absorptive capacity. A firm's innovation and over-all performance is also a function of its ability to scan and exploit the research and innovation of other economic actors in the country or abroad to generate new or better products and processes. For example, the compression of hard disk by *Seagate*, which is very successful commercially, was derived from the research on giant magneto resistance by two Nobel laureates in physics from France and Germany. A firm cannot take advantage of the innovative ideas of other economic agents without the absorptive capacity to understand the basic science and potential commercial application of these ideas. I measured the start-up's technological capacity with prior performance of R&D and the number of patents it owned and/or produced. Because current technological capacity is a good predictor of future innovative activities and outputs, I expect reviewers of SBIR grant applications to favor small business start-ups that have engaged in R&D and/or have produced intermediate innovation outputs like patents.

4.3.3.4. Industry

The value of R&D and innovation varies from one industry to another. Thus, new technology influences post-entry performance of firms differently. In terms of program selection, I expect SBIR funds of the top five participating agencies (i.e., DOD, DHHS, NASA, DOE, and NSF) to accrue disproportionately to small businesses that propose to perform R&D in areas aligned with the federal missions and mandates of these agencies. Based on Black (2004) and Feldman (1994), I created the following seven categorical variables on industrial classification: (1) pharmaceuticals, (2) chemicals, (3) machinery, (4) electronics, (5) electrical equipment, (6) medical and surgical equipment, and (7) R&D and engineering services with other sectors as the omitted or reference category. I expect small business start-ups operating within these seven high-technology sectors to have greater propensity to apply and be selected into the SBIR program and better post-entry performance than their counterparts from traditional sectors.

4.3.3.5. Geographical/Locational Effects

Finally, geographical context matters in the post-entry performance of small business start-ups. Empirical studies have shown that R&D spillovers are prevalent and their magnitude may be quite large. For example, Jaffe (1986) estimated that firms generated on average 0.60 patent per $1 million of R&D expenditure of other firms. More specifically, Jaffe, Trajtenberg, and Henderson (2002) found that R&D spillovers are localized, i.e., firms from the same state or metropolitan region benefit from each other's innovation. Knowledge spillovers are localized because knowledge is sticky (von Hippel, 1998). Firms need both explicit and tacit knowledge as they go about thinking of new products and processes that can strengthen their competitive advantage. Tacit knowledge, in contrast to explicit knowledge, lacks extensive codification and thus is not easily transferable. When knowledge is sticky, the degree of difficulty and cost of transfer are high. This is so because learning is not just gaining new information; it is more about building new competencies and learning new skills and applications, which can be accomplished through "learning-by-interacting" (Lundvall, 1992). The transfer of tacit knowledge is thus higher in states, regions, or local innovation systems where the intensity of R&D by firms, universities, and government laboratories is also high. Greater R&D intensity also attracts highly skilled technical manpower further improving the efficiency of conducting R&D and other innovative activities. I thus expect start-ups that are located in states that spend more in R&D to have greater propensity to develop innovative ideas, prepare stronger SBIR research grant proposals, receive SBIR funding, and perform better post-entry than their counterparts in states that are less known for their R&D activities (e.g. Wyoming and South Dakota).

CHAPTER 5

DESCRIPTIVE STATISTICS AND THE SBIR PROGRAM SELECTION MODEL

Chapter Five presents descriptive statistics and results of the SBIR treatment selection model. The descriptive analysis discusses the characteristics of small business start-ups prior to receiving SBIR financing and compares and contrasts the same with that of more than 4,000 potential control firms from the KFS sample. The treatment selection analysis using a logistic regression model identifies important characteristics of small business start-ups that contributed to successful SBIR application, selection and participation.

5.1 The Treated Sample

Table 5.1 on page 63 presents the baseline characteristics of the 25 SBIR-financed small business start-ups using data from the Kauffman Foundation and the Small Business Administration. Most small business start-ups that received R&D grants from SBIR had at most one employee when they started operation in 2004. Only 28 percent of recipient start-ups had at least two employees and only one hired more than ten employees initially. The median and mean number of employees of the treated sample are one and 1.7 employees respectively.

Table 5.1 Baseline Characteristics of Twenty Five SBIR-Financed Small Business Start-ups

Baseline Characteristics (2004)	Mean	Minimum Value	Maximum Value	Standard Deviation
Firm Size				
Number of Employees	1.68 (1.00)	0.00	15.00	3.17
Human Capital				
Post-Graduate Education	0.80	0.00	1.00	0.41
Industry Experience	14.4 (15.0)	0.00	30.00	9.24
Technological Capacity				
Prior R&D Performance	0.68	0.00	1.00	0.48
Number of Patents	3.24 (0.00)	0.00	35.00	7.22
Positive Sales	0.65	0.00	1.00	0.49
High-Tech Industry				
Pharmaceutical	0.08	0.00	1.00	0.28
Chemicals	0.08	0.00	1.00	0.28
Machinery	0.08	0.00	1.00	0.28
Electronics	0.24	0.00	1.00	0.44
Electrical Equipment	0.04	0.00	1.00	0.20
Medical/Surgical Equipment	0.12	0.00	1.00	0.33
R&D Services	0.28	0.00	1.00	0.21
Geographical Location				
Location in R&D Intensive States (e.g. CA, MA)	0.80	0.00	1.00	0.41
SBA Data				
Minority Ownership	0.52	0.00	1.00	0.51
Women Ownership	0.56	0.00	1.00	0.51

Note: Statistics in parentheses are median values.

Eighty percent of the first owners of SBIR recipients have at least a postgraduate degree. The median is the master's degree category and the mode is more impressive at the doctorate degree category. See Table 5.2 on page 65. The owners of recipient start-ups are not only highly educated but also have vast and extensive industry experience. Seventy two percent of owners have at least ten years of experience in the same industry as his firm is competing in. Only one out of 25 owners did not have any industry experience. The mean and median length of industry experience of owners of the treated sample are 14.4 and 15 years respectively.

Seventeen out of 25 (or 68 percent of SBIR recipients) conducted R&D right at the start of their operation in 2004. In terms of intermediate outputs, close to one-half of the treated sample already had a patent before the treatment period. See Table 5.3 on page 65. Ownership of patents indicates that several treated start-ups might have been spin-off firms from larger firms or new firms established by academic scientists and engineers who had rights to these patents prior to the start-ups' formation. Of treated start-ups with at least one patent, 83 percent had more than one patent and 25 percent had more than five patents. Three treated start-ups had 8, 11, and 35 patents respectively. R&D performance and ownership of patents at the start of business operations can signal potential for future innovations.

Seven SBIR recipient start-ups are operating in R&D and engineering services and six are electronics firms. The other 40 percent are in surgical and medical equipment (12 percent), pharmaceuticals (8 percent), chemicals (8 percent), machinery (8 percent), and electrical equipment (4 percent). Other SBIR recipients (8 percent) are in broad woven fabric mills and business support services.

Table 5.2 Distribution of Level of Education of Owners of SBIR-financed Small Business Start-ups

Level of Education	Valid Percent	Cumulative Percent
Bachelor's Degree	8.00	8.00
Some Graduate School but No Degree	12.00	20.00
Master's Degree	36.00	56.00
Doctorate or Professional School	44.00	100.00

Table 5.3 Distribution of Volume of Patents of SBIR-financed Small Business Start-ups in 2004

Number of Patents	Valid Percent	Cumulative Percent
0	52.00	52.00
1	8.00	60.00
2	8.00	68.00
3	8.00	76.00
5	12.00	88.00
8	4.00	92.00
11	4.00	96.00
35	4.00	100.00

Table 5.4 Agency Funding Sources of SBIR-financed Small Business Start-ups

Agency	Valid Percent	Cumulative Percent
DOD only	36.00	36.00
HHS only	24.00	60.00
NSF only	12.00	72.00
DOE only	4.00	76.00
USDA only	4.00	80.00
DOD and NSF	12.00	92.00
HHS and NSF	4.00	96.00
DOD, HHS and NSF	4.00	100.00

Twenty SBIR recipients (or 80 percent of the entire treated sample) located their businesses in R&D intensive states. Twenty one (or 84 percent) are located in metropolitan or urban areas.

Using data from the Small Business Administration, I will also describe the 25 treated firms in terms of their funding agencies and minority and gender ownerships. Twenty SBIR recipients received funding from a single agency, four from two agencies, and one from three agencies. For single-agency SBIR recipients, nine received SBIR R&D grants from the Department of Defense (DoD), six from the Department of Health and Human Services (HHS), three from the National Science Foundation (NSF), and one each from the Department of Energy (DOE) and the Department of Agriculture (USDA). Three recipient start-ups received funding from both DOD and NSF and a fourth start-up obtained the R&D subsidy from both the NSF and HHS. The lone treated start-up with three agency sources of funding received grants from the DOD, HHS, and NSF. Fifty-two percent of the treated start-ups are minority-owned (i.e., at least one of the owners is non-White) and fifty-six percent are women-owned (i.e., at least one of the owners is a woman).

In summary, this analysis not only provides a descriptive account of the types of small business start-ups that received SBIR R&D subsidies, it also suggests areas to which the study's empirical findings can be generalized. The univariate descriptive analysis of SBIR recipients suggests that the study's empirical findings (i.e., the SBIR program selection analysis and the treatment effects analysis) may be limited to start-ups that operate in electronics, R&D and engineering services, medical and surgical equipment, pharmaceuticals, chemicals, and electrical equipment and to start-ups that conduct industrial R&D associated with the agency missions and focus research areas of the DOD, HHS, NSF, and to a limited extent that of the DOE and USDA.

5.2 Comparison of the Treated Sample and Potential Controls

As Table 5.5 on page 68 shows, untreated small business start-ups (that can potentially serve as controls in the treatment effects analyses) differ in a lot of ways from the twenty five start-ups that received SBIR financing in 2007-08.

In terms of human capital, the recipient or treated sample is four times more likely than the untreated sample to have owners with a postgraduate education or training. Moreover, the owner-entrepreneurs of SBIR recipients had longer experience in the industry where the start-up is operating and competing. Graduate training (whether it be a research degree in science and engineering or a professional degree like an MBA) and prior industry experience of the entrepreneur or primary owner are measures of capabilities that help in sensing and seizing opportunities from new technologies, product prototypes, or new services.

Recipient small business start-ups also have a significant initial advantage in technological capacity. SBIR-financed start-ups are more than three times more likely to conduct R&D than the untreated group. Start-ups that received R&D subsidies also had been more productive generating intermediate innovation outputs, specifically patents. On average, treated start-ups have more than three patents at the end of 2004, while a majority of the potential controls did not produce or at least purchase a license for a patent.

Table 5.5 Baseline Characteristics by Treatment Status

Baseline Characteristics (2004)	Potential Controls (n=4,000+)	Treated Small Business Start-ups (n=25)	Difference	p-value
Firm Size				
Number of Employees	1.94	1.68	0.26	0.840
Human Capital				
Post-Graduate Education	0.20	0.80	-0.60	0.000
Industry Experience	0.55	0.72	-0.17	0.095
Technological Capacity				
Prior R&D Performance	0.21	0.68	-0.47	0.000
Number of Patents	0.15	3.24	-3.09	0.000
Positive Sales	0.91	0.65	0.26	0.000
High-Tech Industry				
Pharmaceutical	0.01	0.08	-0.07	0.000
Chemicals	0.02	0.08	-0.06	0.014
Machinery	0.04	0.08	-0.04	0.350
Electronics	0.04	0.24	-0.20	0.000
Electrical Equipment	0.01	0.04	-0.03	0.204
Medical/Surgical Equipment	0.002	0.12	-0.118	0.020
R&D Services	0.20	0.28	-0.08	0.346
Geographical Location				
Location in R&D Intensive States (e.g. CA, MA)	0.84	0.80	0.04	0.594

A significantly larger proportion of SBIR-backed start-ups are in the fields of pharmaceuticals, chemicals, electronics, and medical/surgical equipment. A larger percentage of treated start-ups are also operating in other high-tech areas like machinery, electrical equipment, and R&D and engineering services, but the differences in proportions between recipient and non-recipient start-ups are not significantly different from zero.

Untreated start-ups, in contrast, have an advantage over SBIR recipients in employment size, sales performance, and location in R&D intensive states. Ninety one percent of the potential controls sold goods and/or services in 2005 compared to only 65 percent of SBIR-financed small business start-ups. The 25 percentage point advantage of untreated start-ups over their treated counterparts is statistically significant ($p<0.001$). However, the same cannot be said of firm size and location advantages of non-recipient start-ups. On average, untreated and treated start-up had 1.9 and 1.7 employees in their initial year of operation, but this difference is both substantively and statistically negligible ($p<0.85$). The potential controls are four percentage points more likely to locate their operations in top R&D performing states like California and Massachusetts than did SBIR recipients, but this difference is also not statistically significant ($p<0.60$). We cannot rule out the possibility that such difference across treatment status is due to random chance.

5.3 SBIR Treatment Selection Analysis

This section identifies the most important firm-level characteristics that contribute to successful application and selection into the SBIR program. The analysis is interesting for many reasons, chief of which is the fact that the program selection model involves small firms that were new to the industry at that time and thus had no prior track record or established reputation to stand on. A track record of success (or at least a strong indication of potential to succeed) is important in securing scarce R&D resources. Table 5.6 on page 72 shows the empirical results of the treatment selection analysis, reporting logit coefficients as well as unstandardized and standardized odds ratios.

In the sample, employment size has a negative effect on the probabilities of being awarded an SBIR grant in 2007-08. Start-ups with more employees are less likely to be selected into the SBIR program. The estimated standard error, however, is too large to generalize such a conclusion from the sample back to the larger population from which the KFS sample was drawn. The same is true for the industry experience of the owners/entrepreneurs: it had the expected sign but the estimated logit coefficient is also not statistically significant. The estimate of its impact or effect is less than two standard errors from zero, indicating that random chance or variation cannot be ruled out as an explanation for the difference in the likelihood of being awarded an SBIR subsidy between start-ups that have owners with at least 10 years of industry experience and those that do not, all things being equal. It is possible that this type of human capital of small business start-ups do not have an effect on the odds and probabilities of receiving an SBIR award.

The level of education of the start-ups' owners has a positive impact on the likelihood of receiving an SBIR subsidy. The odds that a start-up whose owner has a postgraduate degree or training will receive an SBIR grant are 7.3 times as high as the odds of a start-up without an owner with such advanced academic training, all things being equal. Conducting R&D at the start of operations also predicts a start-up's selection into the SBIR program. A start-up's odds of receiving SBIR if it performed prior R&D are 3.6 times as high as the odds of a non-R&D performing start-up, holding the other variables in the selection model constant. The number of patents a start-up possessed at the initial year of operation also positively impacts the likelihood of being granted an R&D subsidy from the SBIR program. As the number of patents rises by one, the odds of receiving an SBIR award rises by 4 percent, *ceteris paribus*.

SBIR selection is also a function of the type of industry where the start-up operates and competes. As expected most industries that are classified as high-tech have a significant advantage in securing SBIR funds over traditional sectors like agriculture and mining and the services sector like education and banking and finance. The odds of a start-up operating in the pharmaceuticals, chemicals, machinery, electronic, electrical equipment, and medical/surgical equipment industries of receiving an SBIR grant respectively are 26.5, 27.3, 15.4, 25.6, 20.6 and 186.5 times as high as the odds of a start-up in the low-technology sector. The differences in the odds of six high-tech sectors and the traditional sectors, which is the omitted category, are all significant at the 5 percent level. In contrast, start-ups in R&D and engineering services have no significant odds advantage in securing SBIR subsidy over traditional and service sectors.

Table 5.6 SBIR Program or Treatment Selection Model

Variables	Logit Coefficient (b)	Odds Ratio (e^{b})	Standardized Odds Ratio (e^{bStdX})	Z-statistics	p-value
Firm Size					
Number of Employees	-0.03	0.97	1.20	-0.48 (0.068)	0.634
Human Capital					
Post-Graduate Education	1.99	7.33	2.27	3.63 (0.549)	0.000
Industry Experience	0.22	1.24	1.11	0.43 (0.507)	0.667
Technological Capacity					
Prior R&D Performance	1.28	3.60	1.70	2.53 (0.506)	0.011
Number of Patents	0.04	1.04	1.10	1.87 (0.088)	0.061
Positive Sales	-1.15	0.32	1.38	-2.23 (0.518)	0.026
High-Tech Industry					
Pharmaceutical	3.28	26.51	1.33	3.03 (1.081)	0.002
Chemicals	3.31	27.27	1.50	3.12 (1.059)	0.002
Machinery	2.74	15.45	1.73	2.51 (1.091)	0.012
Electronics	3.24	25.64	1.82	3.74 (0.808)	0.000
Electrical Equipment	3.02	20.55	1.40	2.38 (1.271)	0.017
Medical/Surgical Equipment	5.23	186.51	1.30	4.57 (1.144)	0.000
R&D Services	1.38	3.98	1.75	1.60 (0.861)	0.109
Geographical Location					
Location in R&D Intensive States (e.g. CA, MA)	-1.00	0.37	1.44	-1.77 (0.564)	0.077

N=3,886, LR =103.49, Prob>LR=0.000. Standard errors are in parentheses.

Geographical location is statistically significant at the less restrictive 10 percent level.[30] The odds of a start-up that is located in R&D intensive states like California and Massachusetts receiving an SBIR grant are only 0.37 times as high as the odds of a start-up operating in states that conduct less R&D. This result may provide empirical evidence for the distributional function of the SBIR program. SBIR R&D subsidy grants are more likely to be distributed to small business start-ups that lack the advantage of knowledge spillovers from intense research and development activities of universities, research laboratories, and firms within their respective local innovation systems.

Using standardized odds ratios, the postgraduate education of the start-ups' owners has the strongest impact on the odds of being selected into the SBIR program, followed by operating in the electronics sector. Among covariates with significant logit coefficients, the number of patents that a start-up possessed prior to application appears to have the weakest impact; a one standard deviation increase in the volume of patents increases the odds of receiving SBIR only by 10 percent. Having sales, on the other hand, decreases the odds of being granted an SBIR subsidy by 38 percent.

Before I discuss the effect of covariates on the probabilities of being selected into the SBIR program, a digression into a few fundamental assumptions of the logistic regression model may be in order. The logistic regression model estimated by maximum likelihood estimation (MLE), in contrast to the linear probability model (LPM) estimated by Ordinary Least Squares (OLS), assumes a nonlinear and non-additive effect of the

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[30] Assuming the null hypothesis, there is one out of ten chance of concluding that the odds advantages are real when in fact there are no differences between the odds and probabilities of receiving SBIR funds between the two geographical locations in the larger population, holding the rest of the variables in the selection model constant.

explanatory variables on the probability of receiving an SBIR grant. LPM's linearity assumption means that a one-unit change in one of the covariates, say X_1, has a constant marginal effect on the predicted probabilities of the dichotomous outcome variable. A nonlinear assumption that the effect of X_k on predicted probabilities would be larger near the floor or ceiling than near the middle makes more theoretical sense. In a model explaining selection into the SBIR program, it is more reasonable to assume that an increase in patents from 3 to 4 will have a smaller impact on predicted probabilities of SBIR participation than an increase in patents from 0 to 1 or 34 to 35. LPM's additivity assumption means that the predicted values of the dichotomous outcome variable depend on the sum of the marginal effects of all explanatory variables from X_1, X_2 to X_k.[31] A non-additive assumption also make much more theoretical sense because it seems more likely that if one of the explanatory variables (say X_1) has reached a sufficiently high level to push the predicted probability near 0 or 1, the effects of other covariates from X_2 to X_k cannot have much influence. In the SBIR selection model, if the number of patents reaches a sufficiently high level, it is very reasonable to assume that the other Xs will have little influence on predicted probabilities of SBIR selection. This is the same as saying that the effect of X_k on predicted probabilities depend on the prior values of X_k.

Because the effect on probabilities from a one-unit increase in X_k is nonlinear and non-additive, there are multiple ways of presenting probabilities changes in logistic regression analysis. Most of these methods calculate probability changes as X_k increases by one-unit while holding the rest of the variables in the model constant at their means

[31] At extreme values of the Xs, LPM's additivity assumption may force predicted probabilities above 1 or below -1, outside the range of a probability.

(Lewis, 2012). Since most of the explanatory variables in the SBIR selection model are dichotomous or a group of dummy variables, it is not meaningful to hold these variables at their means. Instead, I chose a base start-up, calculated its predicted probability of being selected into the SBIR program, and changed one or more variables at a time. The base start-up is a small firm that is owned by an individual with advanced postgraduate training, performed R&D at the initial year of operation, had three patents in the same period, and is operating or competing in the electronics sector. The predicted probability of this start-up is 21.0 percent. The predicted probability of a start-up with the same set of characteristics but without an owner who has an advanced academic training is 3.5 percent or a drop of 17.5 percentage points. A start-up with the same set of characteristics but did not perform R&D only has a 6.9 percent chance of being awarded an SBIR subsidy or a decrease in probability by 14.1 percentage points. A start-up that differs with the base start-up only on the fact that it operates in the low-technology sector has a predicted probability of only 0.6 percent or a drop of 20.4 percentage points. These effects on probability changes are consistent with the effects on the odds of being selected into the SBIR program.

In addition to the tests of single coefficients using the Wald test, I also performed a significance tests for three groups of variables. See Table 5.7 on page 76. The test of human capital variables indicates that these variables are jointly significant at the 0.01 level. We have sufficient evidence from the sample data that not all human capital variables (i.e., education of owner, industry experience of owner, and number of employees) have zero effect on the odds and probabilities of receiving SBIR award. The same can be said of the technological capacity variables (i.e., prior performance of R&D,

volume of patents, and sales). Finally, the seven industrial dummies are jointly significant; at least one of the high-tech sectors have a differential effect on the odds and probabilities of SBIR selection relative to that of the traditional sector and the service sector.

The likelihood ratio test rejects the null hypothesis that all the variables included in the logistic regression model have no effect on SBIR selection. See Table 5.6 on page 72.

Table 5.7 Significance Tests of Groups of Covariates

Groups of Variables	Degree of Freedom	LR Chi-square statistics (χ^{2*})	Prob>χ^{2*}
Human Capital Variables including Number of Employees	3	13.78	0.003
Technological Capacity Variables	3	15.25	0.002
Industrial Classification Dummy Variables	7	31.94	0.000

Note: The null hypothesis is the logit coefficients of the variables are simultaneously zero.

5.4 Discussion

Financially successful start-ups are significantly different from the typical or average start-up (Shane, 2008). The application for public financing for R&D specifically SBIR grants tells the same story: those who applied for and were eventually granted with SBIR funds are significantly different from the typical start-up that started operation in 2004.

As expected, the training and education ($p<0.001$) of owners of small business start-ups significantly predict SBIR selection. As previously discussed in more detail in the previous chapter, the education of the entrepreneurs captures the cognitive ability to sense and seize technological opportunities that others may fail to perceive as both technically feasible and commercially promising. These promising technology research areas are pursued and proposed by highly educated entrepreneurs and are also more likely to have been judged technically and commercially sound by grant reviewers of SBIR participating agencies. Because the breadth of one's training and education can increase the ability to combine unrelated concepts to create something that consumers value, it is thus plausible that highly educated entrepreneurs are more creative, more sophisticated in packaging R&D grant proposals, more technically savvy in pointing to the technological gaps that their proposed R&D will fill, and thus, tend to be more successful in SBIR application and selection. Secondly, the entrepreneur's advanced level of education can also serve as proxy for the extent of his or her network in the scientific or academic community. The priority research areas of SBIR participating agencies are not created in a vacuum; they consider technical inputs from academic scientists and engineers as well as entrepreneurs in the high-technology business sector. Highly educated entrepreneurs are more likely to interact with this network of scholars/researchers and high-technology entrepreneurs, and thus, may be more likely to spot opportunities within current priority SBIR research areas. For entrepreneurs who used to be a member of the academic and scientific community (as Ph.D. students and/or university professors/researchers), it is plausible that they may have directly or indirectly provided inputs to SBIR research areas.

As also expected, performing R&D ($p<0.05$) and owning knowledge assets, specifically patents ($p<0.10$), increase the likelihood of receiving SBIR grants. There are at least two reasons for this empirical result: internal and external. First, those who perform R&D are more likely to sense technological dead-ends, and thus, are more likely to propose technologically sound SBIR proposals. This ability to separate technically promising areas from technological dead-ends, which can be acquired by performing R&D right at the start of business operations, increases the probability of SBIR funding. The external reason has something to do with the reputation of the proponent small firm. Reviewers of SBIR grant applications are more likely to favor proponents who have engaged in R&D, believing that R&D experience increases the firm's absorptive capacity, which enhance success in producing innovations from R&D grants. In addition to indicating successful innovation record, owning patents may further encourage firms to apply for R&D grants. Because innovation is highly complex, that is, it might take a combination of multiple patents to produce a product, process, or service that consumers value, it is plausible that patent-owning start-ups are thinking of generating new patents (out of the public R&D grant), which they will combine with what they currently own to generate innovation. In sum, patent owners are more likely to sense they need a portfolio of knowledge assets to produce innovation, and thus, are more likely to exploit external R&D resources (e.g. SBIR grants) in order to be more successful in orchestrating inputs for innovation.

The industry where the start-up chose to compete or operate significantly predicts the probability of SBIR participation. The odds of receiving SBIR funds of small business start-ups in the pharmaceutical ($p<0.01$), chemical ($p<0.01$), machinery ($p<0.05$), electronics ($p<0.001$), electrical equipment ($p<0.05$), and medical and surgical equipment ($p<0.001$) are at least 15 times as high as the odds of those in the traditional sectors including the services sector. Of course, this empirical result is hardly surprising.

The goal of the SBIR is to stimulate technological innovation, specifically along the mission areas of the 11 participating SBIR agencies. The seven high-tech industries are more likely to correspond with the federal missions and mandates of at least the top five SBIR agencies: DOD, HHS/NIH, NASA, DOE, and NSF. Ninety-six percent (i.e., 24 out of 25) of SBIR recipient small business start-ups obtained their SBIR R&D funding either from the DOD, HHS/NIH, NSF and DOE or a combination of these.

The hypothesis that firm size ($p<0.70$) positively contributes to SBIR selection is not supported. A possible reason is that basic technology research by start-ups is owner-specific. The quality of SBIR grant proposals may depend on the owner-entrepreneur more than his or her own employees. The industry experience ($p<0.70$) of the owner, however, does not matter in SBIR selection. While we hypothesized that more experienced entrepreneurs were more likely to bring with them best-practice organizational routines that are important in running day-to-day operations including R&D, these routines may not be that important in developing quality proposals and therefore in obtaining SBIR awards. Surprisingly, start-ups without sales ($p<0.05$) are more likely to receive SBIR awards. I can proffer at least two explanations. First, start-ups that are looking at long-term R&D as their source of future competitive advantage are more likely to forego production and sales in favor of more R&D. Second, small firms without any short-term inclination or plan to sell goods and services are being created by opportunistic entrepreneurs just for the sole purpose of securing SBIR funds. These two explanations/hypotheses can be tested in future research on SBIR recipient firms.

Finally, there is some evidence that geographical location ($p<0.10$) matters, but surprisingly, it works at the opposite direction, that is, start-ups in states that are known for R&D and innovative activities are less likely to receive SBIR grants. The literature on knowledge and technological spillovers (Jaffe & Trajtenberg, 2002) predicts that the innovating firm benefits from the R&D conducted by universities, government research laboratories, and other firms within its local innovation system. These spillovers will enhance the quality of firms' R&D including their proposal for public R&D grants. It appears that a different mechanism might be at play here. First, start-ups in less R&D intensive locations may correctly perceive that they are at a disadvantage (due to less technological spillovers) and decide to conduct more R&D on their own with the help of federal R&D grants. Thus, it is plausible that start-ups at less R&D intensive states are more likely to apply for SBIR grant in order to conduct R&D on their own instead of relying on research spillovers, which may or may not come (Feldman, 1994). Second, SBIR participating agencies may also sense that small firms in locations with few technological spillovers are at a disadvantage and may decide to distribute SBIR awards evenly between R&D intensive states (e.g. CA and MA) and those that are not well known for their R&D activities, without having to sacrifice the quality of funded SBIR R&D projects. The empirical finding that SBIR funding (at least for small business start-ups) are geographically distributed or dispersed is important. It can offer a political explanation why the SBIR continues as a federal technology program while others like the Advanced Technology Program (ATP) have been terminated. Start-ups from states (e.g. Wyoming and South Dakota) that are not known for their R&D may also benefit from the SBIR program.[32] Specifically for the KFS-SBA dataset, start-ups from Utah,

[32] South Dakota and Wyoming, ranked 50th and 51st in R&D performance respectively, spent only $149

Vermont, South Carolina, and Montana also received SBIR funding.[33] Elected political representatives in the U.S. Congress are more likely to support public programs that benefit their respective constituencies.

million and $98 million in 2004. In contrast, California spent $59.6 billion in R&D in the same period.

[33] Montana is ranked 48th in R&D performance, spending only $295 million in 2004. The 25th ranked state, Missouri, spent $ 3 billion in R&D in the same period.

CHAPTER 6

TREATMENT EFFECT ON POST-ENTRY PERFORMANCE

Chapter Six discusses the empirical evidence on the treatment effect of the SBIR program on the innovation effort, external capital infusion, and other metrics of post-entry performance like sales and employment size of small business start-ups.

6.1 Outcome Variables by Treatment Status

Table 6.1 on page 84 presents statistics on binary outcome variables by treatment status. Except for two variables (i.e., borrowing from banks and licensing out of patents in 2009), all differences in proportions with respect to relevant outcome variables between treated and untreated small business start-ups are statistically significant at the 1 percent level.

While SBIR recipients are 9.2 percentage points more likely to license out a patent in 2009 than non-recipients, the difference is not significant at the 5 percent level. Table 6.3 on page 86 shows the ratio of the odds of licensing out a patent between recipient and non-recipient start-ups, which is also not statistically significant. Treated and untreated start-ups are also equally likely to borrow from banks in 2009, one year after the treatment period (odds ratio =0.95, $p<0.97$).

Focusing only on outcome variables that are significantly different across treatment status, Tables 6.2.1 to 6.2.4 on pages 85-86 present two-way contingency table analyses of the relationship or association between receiving SBIR funding and (a) R&D performance in 2008; (b) innovation propensity in 2009; (c) licensing-in of patents in

2009; and (d) loan borrowings from government agencies in 2009. Table 6.3 on page 86 also provides the odds advantages of SBIR recipients over non-recipients on the same set of outcome variables.

With a gamma statistic of 0.95, receiving SBIR grant and the performance of R&D in 2008 are strongly positively related. SBIR grantees are 72.6 percentage points more likely to engage in R&D in 2008 than non-recipients ($p<0.001$). The introduction of innovation in 2009 and receiving SBIR in 2007-08 are also strongly positively related in the sample (gamma=0.80). The odds that a SBIR grantee introduced an innovation in 2009 were more than nine times as high as the odds of a non-grantee ($p<0.001$). Interestingly, SBIR recipients were 24.7 percentage points more likely to license in a patent in 2009 than did non-recipients ($p<0.001$). The positive association between the two variables is strong with a gamma statistic of 0.91. Finally, the odds of the treated group borrowing from government agencies was almost 20 times as high as the odds of the untreated group ($p<0.01$).

Table 6.4 on page 86 presents tests of differences in interval-level outcome variables between treated start-ups and the potential control group. On average, the treated subsample spent $691,623 in R&D in 2008 while the potential control group spent only $18,490. This difference is statistically significant at the 0.1 percent level. In terms of intermediate innovation outputs, SBIR recipients had, on average, about a four patent advantage over non-grantees in 2009, and this difference is also statistically significant at the 0.1 percent level. In terms of firm size, non-recipients had grown to about four employees in 2009 while recipients to nine employees, on average. The five-employee advantage in 2009 of the treated group over the potential control group is statistically significant at the 5 percent level.

Table 6.1 Binary Outcome Variables by Treatment Status before Matching

Outcome Variables (2008-09)	Potential Controls	Treated	Difference in Proportions	z statistic	p-value
R&D Performance	0.169	0.895	-0.726	-8.303	0.000
Introduction of Innovation	0.142	0.600	-0.458	-5.819	0.000
Licensing- out of Patents	0.096	0.188	-0.092	-1.09	0.138
Licensing- in of Patents in 2009	0.016	0.263	-0.247	-8.147	0.000
External Capital from Family, Friends, Other Individuals in 2009	0.041	0.000	0.041	0.803	0.789
External Capital from Government Agencies in 2009	0.003	0.056	-0.053	-3.86	0.000
External Capital from Banks in 2009	0.058	0.056	0.002	0.045	0.518
External Capital from Gov't, Banks and Other Financial institutions	0.070	0.167	-0.097	-1.592	0.056
External Capital from All Sources in 2008	0.147	0.176	-0.029	-0.346	0.364
External Capital from All Sources in 2009	0.154	0.200	-0.046	-0.491	0.312

Table 6.2.1 R&D Performance in 2008 by Treatment Status

			Treatment Status 0 Non-recipients	1 SBIR Recipients	Total
R&D Performance in 2008	0 No	Column Percentage	83.1%	10.5%	82.6%
	1 Yes	Column Percentage	16.9%	89.5%	17.4%
Total		Column Percentage	100.0%	100.0%	100.0%

γ=0.9532; chi-square statistic = 68.94, $p<0.000$

Table 6.2.2 Innovation Propensity in 2009 by Treatment Status

			Treatment Status 0 Non-recipients	1 SBIR Recipients	Total
Introduction of Innovation in 2009	0 No	Column Percentage	85.8%	40.0%	85.6%
	1 Yes	Column Percentage	14.2%	60.0%	14.4%
Total		Column Percentage	100.0%	100.0%	100.0%

γ =0.801, χ^2 statistic = 33.86, $p<0.000$

Table 6.2.3 Licensing-in of Patents in 2009 by Treatment Status

			Treatment Status 0 Non-recipients	1 SBIR Recipients	Total
License-out Patents in 2009	0 No	Column Percentage	98.4%	73.7%	98.2%
	1 Yes	Column Percentage	1.6%	26.3%	1.8%
Total		Column Percentage	100.0%	100.0%	100.0%

γ =0.9142, χ^2statistic = 66.38, $p<0.000$

Table 6.2.4 Borrowing from Government Agencies in 2009 by Treatment Status

			Treatment Status		
			0 Non-recipients	1 SBIR Recipients	Total
Borrow from Government Agencies in 2009	0 No	Column Percentage	99.7%	94.4%	99.7%
	1 Yes	Column Percentage	0.3%	5.6%	0.3%
Total		Column Percentage	100.0%	100.0%	100.0%

γ =0.90, χ^2 statistic = 14.88, p<0.000; p<0.06 (Fisher's exact test)

Table 6.3 Odds Ratio by Treatment Status

Outcome Variables (2008-09)	Odds Ratio	z statistic	p-value
R&D Performance	41.7	4.98	0.000
Introduction of Innovation	9.05	4.81	0.000
Licensing- out of Patents	2.18	1.07	0.286
Licensing- in of Patents	22.3	5.69	0.000
External Capital from Government Agencies	19.95	2.73	0.006
External Capital from Banks	0.95	-0.05	0.964
External Capital from Gov't, Banks and Other Financial institutions	2.65	1.53	0.126
External Capital in 2008	1.25	0.35	0.730
External Capital in 2009	1.37	0.49	0.625

Table 6.4 Interval-level Outcome Variables by Treatment Status

Outcome Variables (2008-09)	Untreated	Treated	Difference in Means	t statistic	p-value
R&D Expenditure in 2008	18,490.4	691,622.6	- 671,132.2	-10.71	0.000
Number of Employees in 2009	3.93	9.05	-5.12	-1.82	0.035
Number of Patents in 2009	3.19	7.07	-3.87	-3.41	0.001

6.2 Propensity Score Matching

The bivariate descriptive and inferential analyses using contingency tables and test of differences in proportions and means between treated and untreated small business start-ups through chi-square, z, and t tests are not rigorous because antecedent variables that may covary with both treatment status and relevant outcome variables like R&D expenditures and innovation propensities have not been controlled for. As shown in Table 5.5 on page 68, treated and untreated small business start-ups significantly differed in baseline characteristics that can potentially confound the relationship between treatment status and outcome variables.

I controlled for potential confounders through propensity score matching. I predicted the propensity score (or probability of treatment selection) of all small business start-ups in the KFS sample and match SBIR recipients with non-recipients with the nearest propensity scores using the nearest-neighbor matching algorithm. The propensity score model or treatment selection model fits the data well (likelihood ratio χ^2= 103.49, $p<0.001$).

More than 4,000 start-ups that did not receive SBIR funding are matched with the treated subsample. Consistent with the propensity score theorem (Pearl, 2009), units with identical or nearly identical propensity scores have, on average, the same distribution of covariates, which in this case, are antecedent variables that confound the relationship between receiving SBIR subsidy and firm-level outcomes. Table 6.5 on page 88 presents the test of differences in means and proportions of these explanatory variables. The null hypotheses cannot be rejected at the 5 percent level indicating that the distributions of human capital, technological capacity, geographical location, and industrial classification are not significantly different across treatment status.

Table 6.5 Difference in Means of Covariate after Matching

Baseline Characteristics (2004)	Matched Comparison Group	Treated Small Business Start-ups	Difference	p-value
Firm Size				
Number of Employees	1.25	1.17	0.08	0.891
Human Capital				
Post-Graduate Education	0.68	0.83	-0.15	0.219
Industry Experience	0.82	0.72	0.10	0.394
Technological Capacity				
Prior R&D Performance	0.53	0.61	-0.08	0.529
Number of Patents	1.96	1.94	0.02	0.995
Positive Sales	0.65	0.72	-0.07	0.566
High-Tech Industry				
Pharmaceutical	0.11	0.06	0.05	0.527
Chemicals	0.07	0.11	-0.04	0.577
Machinery	0.07	0.06	0.01	0.828
Electronics	0.21	0.22	-0.01	0.916
Electrical Equipment	0.11	0.06	0.05	0.527
Medical/Surgical Equipment	0.05	0.11	-0.06	0.386
R&D Services	0.19	0.28	0.09	0.444
Geographical Location				
Location in R&D Intensive States (e.g. CA, MA)	0.74	0.72	0.02	0.903

6.3 Treatment Effects Estimates

Tables 6.6.1 and 6.6.2.on pages 91-92 present the results of the treatment effects analyses. For comparison purposes, the analyses provide three treatment effect estimates: (1) estimate from the naïve estimator, derived as the difference in group means between SBIR recipients and all potential controls; (2) estimate from propensity score matching, which is the difference in group means between SBIR recipients and their well-matched non-recipient counterparts; and (3) estimate from OLS regression within common support, i.e., the estimate from fitting a least squares regression using only data from the homogenous sample of recipient start-ups and their observationally similar non-recipient counterparts.

The size of the treated and matched comparison subsamples differ from one model to another. For example, Model I has a total of 75 observations while Model VI only includes 72 cases. As discussed in the methods and data chapters, treatment effect analyses through PSM drop treated or untreated observations whose propensity scores are off common support. All cases with missing values in a particular variable are also excluded from the estimation of the ATT. For example, in Model I, one treated observation was dropped from the analysis because it is off common support (i.e., PSM did not find any counterfactual for this SBIR recipient firm), six had missing values in the outcome variable and any of the covariates balanced in the SBIR selection model and thus were also excluded, and only 57 untreated cases have the same covariate distribution as the 18 treated cases retained in the analysis.

As expected, SBIR recipients are more likely to perform R&D in 2008 than observationally-similar start-ups that did not obtain an SBIR R&D subsidy. Focusing on the PSM estimator, 19 out of 57 matched non-recipients (or 33 percent) performed R&D in 2008 compared to almost 89 percent of SBIR recipients. This 56 percentage point difference in the probability of small business start-ups to conduct R&D in 2008 is statistically significant at the 0.1 percent level. The odds of an SBIR recipient performing R&D in 2008 is 16 times as high as the odds of a non-recipient ($p<0.001$), holding constant human capital, technological capacity, geographical location, and industrial classification. The OLS estimate of the same probability difference is close at 49 percentage points ($p<0.001$).

How much is the actual R&D expenditure advantage of treated start-ups? Without propensity score matching, the estimate of the advantage is \$672,092 ($p<0.001$). After balancing the covariates, the treatment effect estimate is reduced to \$477,900, but it remains statistically significant at the 5 percent level. On average, SBIR recipient start-ups spent \$663,379 while their observationally similar non-recipient counterparts spent only \$185,479. See Figure 6.1 on page 92. The OLS estimate of the R&D expenditure advantage of SBIR recipients over non-recipients is slightly smaller at \$442,412 ($p<0.05$). In Model III in which the outcome variable is the natural logarithm of the total R&D expenditure in 2008, SBIR recipient start-ups spent at least 234 percent more in R&D than their observationally similar non-recipient counterparts.

SBIR recipients also have a decisive advantage over their observationally-similar non-recipient counterparts in the introduction of product and process innovations in 2009. PSM and OLS estimate that SBIR-financed start-ups are 33 and 39 percentage points respectively more likely to introduce innovation in 2009 than start-ups not supported by the R&D subsidy program for small businesses ($p< 0.01$). The odds of the treated subsample in introducing innovation are about four times as high as the odds of the matched untreated subsample.

Table 6.6.1 Average Treatment Effects on the Treated (ATT) Estimates: R& D and Innovation

Models	**Outcome Variable**	**Treated Subsample**	**Matched Untreated Subsample**	**Total Sample Size**	**Treatment Effect Estimate**		
					Naïve Estimator	PSM Estimator	Regression within Common Support
Model I	R&D Performance in 2008	18	57	75	0.73*** (8.57)	0.56*** (4.80)	0.49*** (5.15)
Model II	R&D Expenditure in 2008	18	57	75	672,092*** (10.31)	477,900* (2.00)	442,412* (1.97)
Model III	Log R&D Expenditure in 2008	12	35	47	3.56*** (6.73)	2.55*** (3.74)	2.34** (3.36)
Model IV	Innovation Propensity in 2009	18	49	67	0.47*** (5.51)	0.33* (2.18)	0.39* (2.36)
Model V	Licensing-out of Patents	13	26	39	0.09 (0.098)	0.19+ (1.46)	0.16 (1.25)
Model VI	Licensing-in of Patents	18	54	72	0.25*** (8.36)	0.22* (2.20)	0.16* (1.72)
Model VII	Patent Size	11	17	28	3.98** (3.38)	1.34 (0.75)	0.83 (0.40)
Model VIII	R&D Performance in 2009	17	52	69	0.73*** (8.38)	0.43*** (3.96)	0.41*** (3.55)
Model IX	R&D Expenditure in 2009	17	55	72	609,597** (2.47)	270,388 (0.48)	446,644+ (1.33)

Note: significant at ***0.1%, **1%, *5%, and +10%; numbers in parentheses are t-statistics

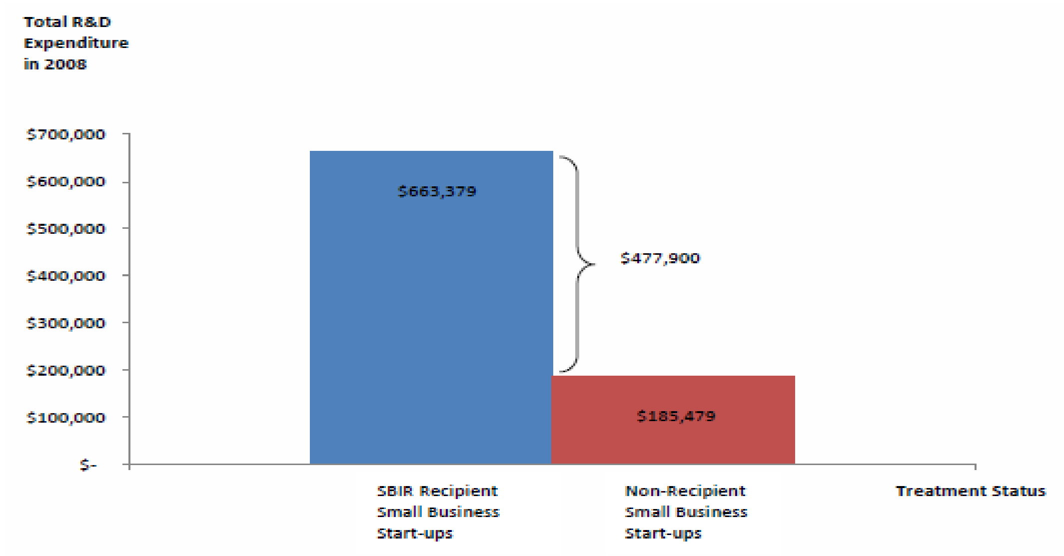

Figure 6.1 Additionality Effect of the SBIR Program

Table 6.6.2 Average Treatment Effects on the Treated (ATT) Estimates: External Capital and Other Outcome Variables

Models	**Outcome Variable**	**Treated Subsample**	**Matched Untreated Subsample**	**Total Sample Size**	**Treatment Effect Estimate**		
					Naïve Estimator	PSM Estimator	Regression within Common Support
Model X	Employment Size in 2009	18	53	71	5.36* (1.94)	7.28** (3.04)	6.09*** (3.69)
Model XI	External Capital from Banks and Non-bank financial institutions	17	53	70	0.10* (1.72)	0.12 (1.27)	0.08 (1.02)
Model XII	External Capital from All Sources	11	34	45	0.05 (0.55)	-0.05 (-0.43)	-0.12 (-1.04)
Model XIII	Sales in 2009	18	54	72	-0.01 (-0.13)	0.14 (1.23)	0.09 (1.24)
Model XIV	International Sales in 2009	14	45	59	0.36** (3.88)	0.11 (0.67)	0.06 (0.40)
Model XV	Profit in 2009	17	52	69	-0.08 (-0.67)	0.01 (0.10)	0.01 (0.05)

Note: significant at ***0.1%, **1%, *5%, and +10%; numbers in parentheses are t-statistics

I also examined the propensity of small business start-ups to license-in external patents and to license-out their own patents in 2009. The treatment effect estimate without matching is an 9 percentage point advantage of treated small business start-ups in licensing-out their own patents to other firms, but is not statistically significant ($p<0.50$). After balancing the confounders, the estimated average treatment effect on the treated is substantially higher at 19 percentage points and is now statistically significant at the 5 percent level. A very interesting finding is that SBIR recipients are more likely to license-in external patents. After balancing the data, the treatment effect of SBIR financing on the probability of licensing-out own patents is 22 percentage points, which is statistically significant at the 5 percent level. The point estimate of OLS is lower at 16 percentage points but still significant at the 5 percent level due to lower estimated standard error than that obtained from the difference in group means after propensity score matching.

The naïve estimator put the post-treatment employment size advantage of SBIR recipients at 5.4 employees ($p<0.05$). However, when observable characteristics were balanced through propensity score matching, the firm size advantage of SBIR-backed start-ups grew to 7.3 employees ($p<0.01$). On average, the treated subsample had 9.4 employees in 2009 while non-recipients had only 2.2 employees. Least squares regression analysis within common support estimates the size advantage of SBIR recipients at 6.1 employees ($p<0.01$), which is very close to the PSM estimate. In Table 6.5 on page 88, the treated and the matched untreated start-ups, by force of statistical matching, started on an equal footing in employment size. Both started at about one employee in 2004 ($p<0.50$). But after five years, SBIR recipients grew to about nine employees or more than an eight-fold increase. On the other hand, their observationally similar counterparts only managed to grow from about one employee in 2004 to about two employees in 2009, or only a two-fold increase.

Contrary to expectations, the treatment effect estimates of SBIR financing on attracting capital are not statistically significant. SBIR-financed small business start-ups are about 12 percentage points more likely to obtain additional capital from banks, government agencies, and other non-bank financial institutions, but such an advantage is not statistically significant ($p<0.25$). Moreover, the sample data shows that SBIR recipient are even slightly less likely than observationally-similar non-recipient start-ups to obtain capital from all external sources including family, friends, and other individuals[34] ($p<0.80$). When all sources of external capital are taken into account (i.e. loans from family, friends, other individuals, government agencies, banks, and non-bank financial institutions), there is almost no difference in the ability of treated and untreated small business start-ups to attract external capital ($p<0.60$).

Does the effect of SBIR on R&D performance persist one year after the treatment period? SBIR recipients are 41 or 43 percentage points (OL and PSM estimates respectively) more likely to engage in R&D in 2009 than their observationally-similar non-recipient counterparts ($p<0.0001$). On average, SBIR-financed small business start-ups also outspent their non-recipient counterparts by $446,644 (OLS estimate) but such an R&D expenditure advantage is only marginally significant at the 10 percent level due to a smaller sample size and by extension, larger standard errors.

[34] Other individuals include business angels.

6.4 Discussion

Without controlling for variables that covary with both SBIR selection and post-treatment performance, SBIR recipient and non-recipient small business start-ups significantly differ in relevant outcome variables. SBIR grantees are more likely to perform R&D ($p<0.001$), introduce product and/or process innovations ($p<0.001$), license-in external patents ($p<0.001$), obtain external capital from government agencies ($p<0.001$), and borrow from banks and non-bank financial institutions ($p<0.10$) after receiving the R&D subsidy. Publicly-funded start-ups also spend more in R&D ($p<0.001$), had more patents ($p<0.01$), and hired more employees ($p<0.05$).

To make the conditional independence assumption (CIA) or ignorability of treatment assignment assumption plausible in this observational or non-experimental study[35], we balanced the covariates that we think affect both treatment selection and post-treatment outcomes. We constructed a comparison sample that includes non-recipient start-ups that are observationally similar to SBIR recipient start-ups by matching on their propensity scores. Consistent with the propensity score theorem (Pearl, 2009), SBIR recipient and non-recipient start-ups that have almost identical propensity scores are, on average, have the same covariate distribution. After propensity score matching, the matched comparison group and the treatment group are not significantly different in terms of firm size, human capital, technological capacity, industrial classification, and geographical location. It is as if this select group of start-ups had been randomly

[35] For a detailed discussion of the ignorability of treatment assignment, see Chapter 3.

assigned to either the treatment or comparison group[36], making the assumption that treatment assignment is ignorable or independent of outcomes plausible. In this synthetic sample of treated and matched untreated start-ups, firm size, human capital, technological capacity, industrial classification, and geographical location do not predict SBIR selection. Neither group had an advantage in terms of the number of employees ($p<0.90$), the education ($p<0.30$) and industry experience ($p<0.40$) of the owners/entrepreneurs, prior R&D experience ($p<0.60$), number of patents ($p<0.995$), sales ($p<0.60$), industrial classification (at least $p<0.40$), and geographical location ($p<0.95$).

Because the CIA or ignorability of treatment assignment holds, the observed outcome of the untreated group can serve as empirical proxy for the unobserved outcome of the treated group. We then estimated the ATT as the difference in post-treatment outcomes of the treated and matched comparison subsamples.

The certification hypothesis is not supported by the data. We predicted that SBIR recipients would use their SBIR awards to signal the viability of their innovation projects and their respective companies in order to attract external capital. SBIR recipients are 8-12 percentage points more likely to obtain external capital from banks and non-bank financial institutions in 2009, but such external capital infusion advantage is not statistically significant. We also found that non-recipient start-ups are equally likely to obtain external capital from all sources (including family, friends, and other individuals) as SBIR recipient start-ups. It is possible that SBIR funding obviates the need for

[36] In experimental studies, random assignment ensures that outcomes are independent of treatment.

external capital and/or recipient small business start-ups have not sufficiently expanded after five years to warrant external capital infusion.

There is empirical evidence to support the additionality hypothesis using the KFS-SBIR data. SBIR recipient start-ups are 56 percentage points ($p<0.001$) more likely to engage in R&D in 2008 than their observationally similar non-recipient counterparts. More specifically, SBIR grantees, on average, spent $663,379 in R&D in 2008 while matched non-grantees spent only $185,479. Assuming the mean independence assumption[37] (Heckman, 1998), SBIR recipient start-ups would have spent only $185,479 in R&D had they not applied for and obtained subsidy from the SBIR program. The extra $477,900[38] ($p<0.05$) can be construed as the input additionality effect of the SBIR program. The SBIR program raised the R&D effort of recipient start-ups from $185,479 to $663,379. The expectation is better R&D is being conducted at a higher R&D effort of $663,379. Economies of scale support this conclusion. While R&D outputs are not a monotonic function of R&D inputs, an R&D effort of $663,379 most likely would have satisfied Metcalfe's (1995) critical minimum level of R&D effort necessary to produce desired innovation outputs.

There is also indication of the output additionality of the SBIR. Grantees are at least 33 percentage points ($p<0.05$) more likely to introduce product and/or process innovations in 2009. Interestingly, SBIR recipients are about 20 percentage points ($p<0.05$) more likely to purchase a license to use external patents. This could be

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[37] The mean independence assumption only assumes that the untreated outcomes are the same across treatment states, that is, $E(Y_{i0} \mid T_i=1, X_i) = E(Y_{i0} \mid T_i=0, X_i) = E(Y_{i0} \mid X_i)$. See Chapter 3.
[38] The OLS estimate within common support is $442,412 ($p<0.05$).

evidence of the orchestration activities of enterprises to produce innovation and gain a unique competitive advantage. Due to the complexity of the innovation process, the possession of one or two patents may not be sufficient to produce an innovation that will enjoy robust consumer demand. To produce a good or service with high value-added (from the consumer's perspective), the firm may have to combine its own internal knowledge assets with that of external parties: other firms, government laboratories, universities, or individuals. Purchasing a license to use an external patent is a mechanism to outsource complementary assets (Teece, 2009) through open innovation (Chesbrough, 2003).

From Table 6.6.1 on page 91 and Figure 6.1 on page 92, we established that SBIR recipient start-ups would have spent only $185,469 in R&D in 2008 had they not received SBIR program funds. This empirical result prompts the question, what was the actual private R&D expenditure of SBIR recipient start-ups in 2008? On average, SBIR recipients had been granted $553,991 in R&D subsidies in 2008. See Figure 6.2 on page 99. Thus, SBIR recipients spent only $109,398 in R&D in addition to what they received as SBIR subsidy. This is interesting because as we discussed earlier assuming mean independence, SBIR recipients would have spent $185,479 without the SBIR subsidy, but with SBIR, they opted to decrease out-of-pocket R&D expenditure to $109,398, a decrease by more than $75,000, on average. Is this empirical evidence of crowding out? It could be. The infusion of public financing through the SBIR subsidy decreased private contribution to R&D. However, it is entirely plausible that such a decrease in privately-financed R&D only followed from what the recipient firms thought was their "optimal level" of R&D effort for that particular period. After they secured about $550,000 in

SBIR R&D subsidy, they might have thought, correctly or incorrectly, that adding $185,000 (instead of $109,000) would not have mattered. The SBIR subsidy may have already satisfied what the firm perceived as the critical minimum level or optimal level of R&D that they did not find value in adding more private funds. This has important implications for the design of innovation policies and programs like the SBIR.

However, we have to acknowledge that it is possible that the crowding-out effect of the SBIR program has been overestimated. The quantification of privately financed R&D by SBIR recipient start-ups rests on the untestable assumption that the SBIR R&D subsidy of about $554,000,[39] on average, received in 2008 was spent for R&D in the same year. If SBIR recipients only used a portion of the SBIR subsidy in 2008, then the crowding out effect is smaller than what we estimated at about $75,000, on average.

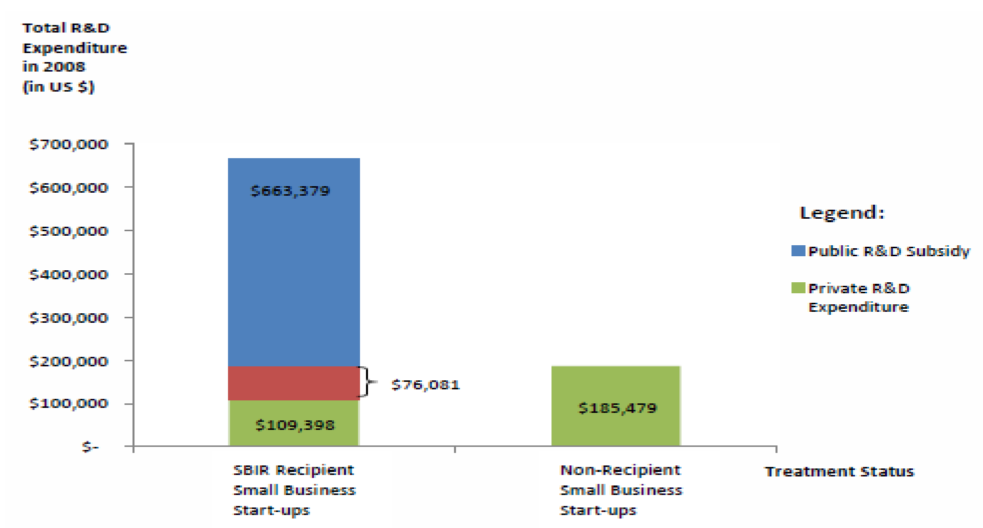

Figure 6.2 Private R&D Expenditure by Treatment Status

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[39] The ability of small businesses to secure SBIR funds from multiple agency sources at different phases of SBIR funding makes it possible for a small business start-up to receive more than $500,000 in R&D subsidy.

Finally, the estimation of the average treatment effect on the treated (ATT) of the SBIR program rests on the assumption that SBIR selection is based on observable characteristics. While we used advances in statistical matching to establish the counterfactual outcomes of SBIR recipients, our method does not guarantee that our treatment effect estimates are bias free. As program evaluators, analysts, and policy researchers, we have to acknowledge that estimates of observational studies or non-experimental studies are always susceptible to bias. In this dissertation, if SBIR selection is a function of unobservables (e.g. motivation of the owner-entrepreneur) or other observable characteristics that the KFS and SBA datasets did not measure and therefore were not controlled for in the selection model, then our estimates may include a bias. Our hope is that this bias is small enough so as not to change our qualitative conclusion that SBIR positively affects R&D effort, innovation propensity, employment size, and the orchestration of internal and external knowledge assets to produce innovation. We have theoretical reasons to believe that this is the case. For illustration purposes, let us assume that SBIR selection is also a function of the quality of the submitted SBIR proposal, which is unobserved. It is reasonable to assume that the distribution of the quality of the SBIR proposal is the same across treatment status, that is, SBIR recipients and observationally similar non-recipients have the same probability of generating quality SBIR proposals. If the treated and matched comparison groups are equally likely to have this unobserved characteristic, the omitted variable does not result to unobserved bias. SBIR recipient and non-recipients who have the same covariate distribution in firm size, education and experience of the owner-entrepreneur, R&D performance, possession of external knowledge assets, sales, industrial classification, and geographical location are more likely to have the same probability of producing quality SBIR proposals than treated and untreated groups that significantly vary in observable characteristics. This is an untestable but very plausible assumption. In matching, observations with the same distribution of observable characteristics are more likely to have the same distribution of unobserved characteristics (Rosenbaum, 2005).

CHAPTER 7

SUMMARY, CONCLUSION, AND THEORETICAL AND POLICY IMPLICATIONS

Chapter Seven provides the conclusions and theoretical and policy implications of this study as well as recommendations for future research. The study's conclusions and implications may be relevant to the Small Business Administration, SBIR participating agencies, the U.S. Congress, other research and innovation policymakers, and small business start-ups.

This objective of this dissertation is to contribute to expanding our knowledge base on the consequences of research and innovation policies and programs. While we agree that these public policies and programs can be construed as technological bets on our collective economic future (Borrus & Stowsky, 1999), the payoffs from these bets should at least be non-negative. As of this writing, the Manchester Institute of Innovation Research (MIoIR) of the University of Manchester has embarked on putting together a compendium of evidence on innovation policy interventions like the establishment of science parks to build local innovative clusters, fiscal incentives for R&D, public venture capital investment, and innovation procurement programs. This dissertation could be seen as part of this larger effort to identify more systematically which policies that encourage and promote innovation work and which do not, specifically by examining in more depth the Small Business Innovation Research (SBIR) program.

This dissertation advances the literature on federal technology policies in general and the SBIR program in particular in at least three ways. First, unlike R&D subsidy studies that focused almost exclusively on EU countries primarily due to the availability of national and regional innovation surveys, this dissertation focused on small business start-ups in the United States using a new scientific survey of new firms. It examined the effect of a federal R&D subsidy program on the innovation effort and other metrics of post-entry performance of start-up enterprises. Second, unlike SBIR studies that used only recipient firms in their empirical analyses, this dissertation built a new dataset, or more precisely, integrated two new datasets that ultimately included both recipient and non-recipient small firms. SBIR studies whose samples did not include non-recipient firms cannot establish the counterfactual outcomes of SBIR recipients had they not applied for and granted R&D funds. In the counterfactual approach to causal inference, the determination that program X causes outcome Y requires that (1) outcome Y was produced in the presence of program X, and (2) outcome Y was not produced in the absence of program X. Both conditions must be established empirically before one can attribute post-program outcomes to the implementation of the program. Using only SBIR recipients does not completely satisfy the second condition. In reflexive studies, pre-program outcomes of recipients cannot serve as their valid and unbiased counterfactual outcomes had they not received the R&D subsidy. This dissertation, as far as we know, is the first effort to establish the counterfactual outcomes of small business start-ups that received SBIR R&D grants. It attempted to achieve this objective in a two-step process: (1) datasets integration, and (2) statistical matching. We requested the Kauffman Foundation permission to use the confidential version of the Kauffman Firm Survey dataset that is available in the NORC data enclave. We also requested the SBA to provide us a SBIR recipient dataset and the Mathematica Policy Research to integrate the same with the KFS. The integrated KFS-SBA dataset, which identified small business start-ups that received SBIR funding, and at the same time, contained thousands of small firms that could serve as potential controls for SBIR recipients allowed us to empirically construct the counterfactual outcomes of SBIR recipients. Third, unlike SBIR studies that manually and artificially combined recipient and non-recipient small firms, this dissertation's sample of treated and untreated small business start-ups was drawn from the same probability sample of new enterprises. Thus, the two groups of small firms came from the same or identical distribution. In addition, this study used advances in statistical matching to achieve better comparability between the two groups of small business start-ups.

For program evaluators who lack experimental data, achieving comparability between treated and control groups, which is a key requirement before one can make meaningful causal inferences, can be implemented through statistical matching, specifically in this dissertation, matching on the propensity score. If it can be shown that program selection is a function of observable characteristics and that these covariates are balanced after PSM, then it could be argued that selection bias is controlled for or at least minimized when estimating the causal effect of a policy, program, or project. Estimates from non-experimental methods (like PSM) can be offered as tentative estimates of the treatment effect of a policy or program until they are confirmed or refuted by more rigorous evaluation methods like an experimental research design or a regression discontinuity design.

This dissertation has shown that small business start-ups that received SBIR R&D grants are significantly different from the typical start-up that did not apply and/or selected for SBIR funding. The entrepreneur-owners of SBIR recipients have more advanced academic training and longer industry experience than the average or typical entrepreneur. Moreover, SBIR recipient-firms have a higher propensity to perform R&D and are more likely to own intellectual property at the start of their business operations in 2004. In short, small business start-ups that eventually received SBIR grants started at a higher technological trajectory than did the typical start-up, an empirical finding that fundamentally proceeded from the fact that most SBIR recipients in the KFS-SBA sample competes or operates in the pharmaceutical, chemicals, machinery, electronics, electrical equipment, medical and surgical equipment, and R&D and engineering industries. However, recipients do not have size and locational advantages over typical start-ups: recipient and non-recipient start-ups started their operations, on average, with close to two employees and are also equally likely to locate their businesses in R&D intensive states like California and Massachusetts. SBIR grantees, though, were less likely to generate sales at their first year of operations than their non-recipient counterparts.

All the significantly different baseline attributes between SBIR recipients and non-recipients, as expected, figured prominently in SBIR selection. The odds of receiving SBIR grant of a small business start-up whose owner has a post-graduate education is more than nine times as high as the odds of a start-up without an owner with the same advanced level of academic training. The odds of being granted SBIR R&D subsidies are also higher for those who had prior R&D experience and for those who had patents at the start of their business operations. Start-ups that are operating in the high-technology sector (i.e., pharmaceuticals, chemicals, machinery, electronics, electrical equipment, and medical/surgical equipment) are also more likely to receive SBIR funds than start-ups in the traditional sectors (e.g. agriculture, mining) and the services sector. Surprisingly, start-ups that did not sell goods and services are less likely to receive SBIR grants. Interestingly, location matters but at a different direction: start-ups located in states that are not known for their R&D performance are more likely to receive SBIR funding. Firm size did not appear to affect the probability of receiving SBIR award.

From the determinants of SBIR program selection, we derive the following theoretical and policy implications and areas for future research.

First, prior R&D and innovation record are a market signal on the ability of start-ups to innovate. It is not entirely accurate that small business start-ups do not have a track record to stand on when applying or competing for scarce public R&D resources against established businesses. Performing R&D right at the start of business operations can signal the start-up's intent to continue performing R&D in the future. SBIR participating agencies judge R&D performers more favorably. Because learning is cumulative, success in producing knowledge assets and innovation in the past underpin future innovation performance. Having patents signals the knowledge and experience the firm's owners have acquired over time. These patents may have been applied for and approved by the United States Patent and Trademark Office (USPTO) prior to the establishment of the firm (e.g. when the owner was still affiliated in a university as a graduate student or a faculty member), but just the same, it sends a credible signal that the small business start-up has learned something substantial in the past and is able to pursue and produce innovation in the future.

Second, there is evidence of the distributional function of the SBIR program. Start-ups in states that are not known for R&D (e.g. Wyoming, South Dakota, Montana) are not less likely than their counterparts in R&D intensive states (e.g. California, Michigan, Massachusetts) to receive SBIR funding. Scholars in political science and policy studies may want to study the factors influencing the dispersion of SBIR awards. Qualitative studies can probe in more depth the decision of SBIR agencies how to distribute SBIR R&D awards geographically. For example, it is possible that the first n percent of SBIR funds goes to the most qualified small businesses and the remaining ($1-n$) percent to the not-so-quality technology R&D proposals but were submitted by small firms in less R&D intensive states. These are conjectures and hypotheses that may be validated or disconfirmed by follow-up studies.

Finally, start-ups that do not generate sales may be more likely to receive SBIR funding. This result can be interpreted in at least two ways. First, those that perform R&D are more likely to forego generating sales; their primary intent is to invest in knowledge asset production before introducing a new product or service in the market. Alternatively, these small firms may have been established primarily for the purpose of securing public R&D grants. They may not have the objective of directly selling goods and services themselves; instead, they use firm formation as a mechanism to siphon federal R&D resources. This is a hypothesis that can be further investigated in greater depth by follow-up studies. From a normative standpoint where the production of knowledge is a policy goal, it is not necessarily economically inefficient to use public resources to do more R&D, produce knowledge assets out of it, and perhaps in the future license these assets out to other firms that may value the R&D outputs (e.g. patents) more than the original researcher or inventor. Using federal R&D resources only for the sole purpose of conducting R&D and producing knowledge assets without the clear intent to directly commercialize these assets may not necessarily be a weakness in the innovation system. This is a fertile area for future research utilizing the NIS approach.

After estimating the SBIR program selection model, we employed propensity score matching to balance pre-treatment characteristics of SBIR recipients and non-recipients. We constructed the comparison sample by identifying non-recipients with nearly identical propensity scores as that of SBIR recipients. Consistent with the propensity score theorem (Rosenbaum & Rubin, 1983; Pearl, 2009), observations with the same distribution of propensity scores have the same distribution of observable characteristics. PSM made the comparison and treatment samples homogenous except in SBIR program exposure, making the fundamental assumption of ignorability of treatment assignment in causal studies more plausible. Achieving or at least improving homogeneity between groups not only reduces variability of the estimates of treatment effects but also their sensitivity to unobserved bias. We also used parametric models as robustness check of the PSM estimates.

Using the realized outcomes of observationally similar non-recipient start-ups as the counterfactual outcomes of SBIR recipients had they not received SBIR funds, we found empirical evidence of the input additionality effect of the SBIR program. Had they not applied for and granted SBIR R&D subsidies, recipient start-ups would have spent only $185,000 in R&D, but with SBIR their R&D effort was significantly increased to

$663,000. The expectation is SBIR recipients are undertaking more risky but higher return innovation projects with the R&D subsidy. The input additionality effect of SBIR is consistent with the findings of Aerts and Schmidt (2008), Czarnitzki and Licht (2006), Gonzalez and Pazo (2008), and Ozcelik and Taymaz (2007) that R&D subsidy programs in Europe, specifically in Germany, Spain, and Turkey, have a positive effect on total R&D expenditure and intensity. Please see Appendix E for a more detailed description of the data, methods, and findings of these R&D subsidy and SBIR evaluation studies.

However, it appears that public co-financing of commercial R&D has crowded-out privately financed R&D of small business start-ups in the United States. Without SBIR funding, recipient start-ups are expected to spend about $185,000 in R&D, but with the R&D subsidy, their privately-financed R&D decreased to $109,000. A dollar of R&D subsidy decreased privately-financed R&D by about $0.16. This finding calls into question the size of SBIR grants and the absence of a requirement for private R&D funds. Receiving more than $400,000 of SBIR grants may have decreased the need to shell out private funds for R&D. This problem may have proceeded from the fact that small firms can receive grants from multiple agency sources. SBA may have to reexamine its policy regarding multiple agencies (e.g. DOD, NIH, NSF) funding the same technology research and the size of the grants. Requiring recipients to shoulder a certain percentage of the R&D project cost may ensure a truly private-public co-financing of early-stage, pre-competitive technology research.

That the SBIR subsidy decreased private R&D expenditures runs counter to the findings of several R&D studies (Hussinger, 2008; Lee & Cin, 2010; Koga, 2005; and Ozcelik & Taymaz, 2008) that public R&D grants induce additional company-funded

R&D activities. Two studies that support this dissertation's finding on public R&D partially substituting for privately financed R&D are Gonzalez and Pazo (2008) who found that public funds do not significantly stimulate private R&D expenditures and Wallsten (2000) who showed that the size of SBIR grants significantly decreased firm-financed R&D of small businesses in the U.S.

This study's estimate of the crowding-out effect of SBIR on privately-financed R&D is smaller than what Wallsten (2000) found using a sample of 81 SBIR recipients and non-recipients. His 3SLS estimate showed that a dollar of SBIR subsidy decreased firm-financed R&D by $0.82. The difference in the magnitude of the crowding-out estimates may be due to differences in estimation methods and the populations from which the samples were drawn. This dissertation focused on the effect of SBIR on small business start-ups while Wallsten (2000) on the general population of small firms that are eligible to apply for SBIR grants. As shown previously, young and small firms may be more innovative than their more established and older counterparts, and thus may be less likely to decrease privately-financed R&D with the receipt of the SBIR subsidy than the latter. The hypothesis that the crowding-out effect of public R&D grants is smaller in start-up enterprises than in older small firms can be tested in future studies.

We also found significant output additionality of SBIR. Recipient start-ups are more likely to introduce process and/or product innovations 1 to 2 years after receiving R&D subsidy. This finding is consistent with Berube and Mohnen (2009), who found that firms that receive R&D subsidies in Canada are more likely to introduce product innovations and generate sizable revenues from them. Future studies may explore the actual mechanisms through which the SBIR grant directly contributed to the production of these innovations.

SBIR recipients grew significantly faster than non-recipients in terms of employment size at least one year after receiving the R&D subsidy. This empirical finding may indicate that SBIR program funds can help in augmenting scientific and technical personnel and possibly, in hiring complementary human resources like marketing researchers.

Contrary to the findings of Lerner (1999) and Toole and Turvey (2009) that the SBIR award positively impacts follow-on venture capital financing, we did not observe any significant "halo effect" or "certification effect" of receiving an SBIR award on attracting external capital, regardless of the source of this external capital. The SBIR subsidy may obviate the need for external private capital. That is, as internal resources for R&D are freed up, SBIR recipients can redeploy these resources for marketing, production, and operations. The new firm may not have grown and expanded enough to warrant external capital infusion. These hypotheses can be further examined in future studies on the medium- and long-run certification effects of the SBIR. Another reason why previous studies found a significant certification effect of the SBIR on external capital while this study did not is sample size. For example, Lerner (1999) found that SBIR recipients were 1.4 percentage points more likely to attract venture capital financing than the matched small firms, and this small effect was statistically significant because the study used a large sample of 1,193 small firms. On the other hand, we found that SBIR recipients are 12 percentage points more likely to attract external capital than non-recipients but such outcome advantage is not statistically significant because of a larger standard error due to a smaller sample size.

While we did not find a significant "halo" effect of SBIR on attracting external capital, we discovered a different certification effect of the program. Start-ups that received SBIR grants are more likely to attract external patents. This finding can indicate that the SBIR award certifies the quality of the company and the innovation project that the recipient is undertaking through the SBIR subsidy. As such, individuals, government laboratories, and companies that own patents may be more willing to license their knowledge assets to this group of small firms that they believe will be more successful in using their patents to produce innovations. More successful use of these external patents by "certified" SBIR grantees translates to a more steady revenue stream of royalties by these patent holders.

That SBIR grantees are more likely to outsource complementary assets is empirical evidence of the orchestration activities for innovation of SBIR recipients. This empirical finding is important for two reasons. At the firm level, it indicates that innovation requires a combination of internal and external knowledge assets. Competitive advantage may not lie in knowledge assets produced by private R&D investment and/or public R&D subsidy. It is now increasingly defined by the firm's ability to orchestrate an internal-external portfolio of knowledge assets (Chesbrough, 2003; Teece, 2009). It will be very interesting to see future studies examining whether start-ups that orchestrated an internal-external portfolio of knowledge assets are indeed more innovative and more successful financially. At the SBIR program level, it is not enough to measure R&D inputs and outputs. It is also critical to look deeper into the orchestration activities of program grantees, that is, how they combine previous knowledge assets with new knowledge assets generated through the SBIR funding, and how they integrate both old and new internal assets with external knowledge assets to produce innovation. Future evaluations of the SBIR should take into account the complexity of the innovation process. New product prototypes that result from the public co-financing of private R&D may not be enough to produce innovations that have high customer value added. The recipient's ability to procure knowledge externally may be a positive effect of the public financing of commercial R&D; the public program may strengthen the grantee's absorptive capacity to use external technologies. These long-run effects will benefit the innovation economy in the long-run, at least from an evolutionary economic perspective.

Our empirical results highlight the importance of looking at innovation policy instruments not solely as "correctives to market failures." Theoretically and empirically, R&D subsidies (which the SBIR program provides) can help recipient-firms satisfy the minimum scale of R&D, close the gap between private and socially optimal level of R&D, and alleviate the risks and uncertainties of the outcomes of the innovation effort, thus correcting market failures in the production of commercially-useful knowledge. But, these policy instruments are more than correctives to indivisibilities, positive spillover effects, and uncertainties. More importantly, they are a mechanism to bring together the state, the private sector, and the R&D community to identify technological challenges, solutions, and breakthroughs together (Whitford & Shrank, 2011). The STTR requirement to small recipient-firms to have a university partner to conduct R&D supported by federal R&D grants is a welcome adjustment to the SBIR.

Increasingly, the SBIR and other innovation policies and programs should be construed as an attempt to apply networks in the governance of the innovation system. Relying on hierarchies and the state machinery to identify the technological need and solutions of the national economy was discredited a long time ago with the collapse of most socialist and communist regimes. Dependence on the private market is also not reliable because the private sector also tends to be myopic: it cannot see all technological possibilities and potential breakthroughs using the profit motive lens alone. It might take the concerted effort of the state, the private sector, and the academic/scientific/research community to govern the production and distribution of knowledge assets that underpin global competitive advantage. Future evaluations should treat the SBIR program in this regard. [R.V.G.]

APPENDIX A

VARIABLE DEFINITIONS – BASELINE CHARACTERISTICS

Variables	Type	Definition
Firm Size		
Number of Employees	Interval	number of employees the start-up had in 2004
Human Capital		
Post-Graduate Education	Binary	coded 1 if the owner/entrepreneur has a master's or doctorate degree, 0 otherwise
Industry Experience	Binary	coded 1 if the owner/entrepreneur has at least 10 years of industry experience, 0 otherwise
Technological Capacity		
Prior R&D Performance	Binary	coded 1 if the start-up performed R&D in 2004, 0 otherwise
Number of Patents	Interval	number of patents the start-up owned in 2004
Positive Sales	Binary	coded 1 if the start-up sold goods and services in 2004, 0 otherwise
High-Tech Industry		
Pharmaceutical	Binary	coded 1 if the start-up is a pharmaceutical firm, 0 otherwise
Chemicals	Binary	coded 1 if the start-up is a chemicals firm, 0 otherwise
Machinery	Binary	coded 1 if the start-up is a machinery firm, 0 otherwise
Electronics	Binary	coded 1 if the start-up is an electronics firm, 0 otherwise
Electrical Equipment	Binary	coded 1 if the start-up is an electrical equipment firm, 0 otherwise
Medical/Surgical Equipment	Binary	coded 1 if the start-up is a medical and surgical equipment firm, 0 otherwise
R&D Services	Binary	coded 1 if the start-up is a R&D and engineering services firm, 0 otherwise
Geographical Location		
Location in R&D Intensive States (e.g. CA, MA)	Binary	coded 1 if the start-up is located in the top 25 R&D intensive states, 0 otherwise

APPENDIX B

VARIABLE DEFINITIONS – OUTCOMES

Variables	Type	Definition
R&D and Innovation		
R&D Performance in 2008	Binary	coded 1 if the start-up performed R&D in 2008, 0 otherwise
R&D Expenditure in 2008	Interval	amount of total R&D expenditure in 2008 (in US$)
Innovation Propensity in 2009	Binary	coded 1 if the start-up introduced new product or process in 2009, 0 otherwise
Licensing-out of Patents in 2009	Binary	coded 1 if the start-up licensed out own patent in 2009, 0 otherwise
Licensing-in of Patents in 2009	Binary	coded 1 if the start-up purchased a license to use external patent in 2009, 0 otherwise
Patent Size in 2009	Interval	number of patents that start-up had in 2009
R&D Performance in 2009	Binary	coded 1 if the start-up performed R&D in 2009, 0 otherwise
R&D Expenditure in 2009	Interval	amount of total R&D expenditure in 2009 (in US$)
External Capital Infusion		
External Capital – Banks and Non-bank in 2009	Binary	coded 1 if the start-up obtained capital from a bank or non-bank financial institution in 2009, 0 otherwise
External Capital – All Sources in 2009	Binary	coded 1 if the start-up obtained capital from a bank or non-bank financial institution, government agencies, family, friends, and other individuals in 2009, 0 otherwise
Employment, Sales, and Profit		
Employment Size 2009	Interval	number of employees the start-up had in 2009
Positive Sales in 2009	Binary	coded 1 if the start-up sold goods and services in 2009, 0 otherwise
International Sales in 2009	Binary	coded 1 if the start-up sold goods and services in the global market in 2009, 0 otherwise
Profit in 2009	Binary	coded 1 if the start-up had a profit in 2009, 0 otherwise

APPENDIX C

SBA TRANSMITTAL LETTER OF SBIR RECIPIENT DATASET

U.S. SMALL BUSINESS ADMINISTRATION
WASHINGTON, DC 20416

April 23, 2010

Mr. Reynold Galope
Georgia Tech
251 10th St. NW, #A-412
Atlanta, GA 30318

Dear Mr. Galope,

This letter responds to your Freedom of Information Act (FOIA) request via email dated March 15, 2010, for information regarding the SBA TechNet database.

We have provided all the information you have requested on the enclosed CD.

Sincerely,

Lisa Younger

Lisa Younger
Program Analyst

Enclosure

APPENDIX D

STATA OUTPUTS OF ATT ESTIMATES

Logistic regression		Number of obs	= 3886
		LR chi2(14)	= 103.49
		Prob > chi2	= 0.0000
Log likelihood = -89.168019		Pseudo R2	= 0.3672

treat	Odds Ratio	Std. Err.	z	P>|z|	[95% Conf.	Interval]
c5_num_emp~0	.9681393	.0659015	-0.48	0.634	.8472203	1.106316
post_grad_~0	7.326652	4.021197	3.63	0.000	2.498779	21.48243
work_exp_o~m	1.243486	.6300084	0.43	0.667	.4606618	3.3566
f19_res_de~0	3.601529	1.820901	2.53	0.011	1.336987	9.701677
patent_0	1.039199	.0213459	1.87	0.061	.9981926	1.08189
d6_have_sa~1	.31545	.1632708	-2.23	0.026	.1143839	.8699534
pharma	26.50903	28.64539	3.03	0.002	3.188555	220.3909
chemicals	27.2729	28.88152	3.12	0.002	3.422344	217.3396
machinery	15.44698	16.85112	2.51	0.012	1.820823	131.0448
electronics	25.63623	22.25904	3.74	0.000	4.675046	140.5797
electric_e~p	20.55	26.12688	2.38	0.017	1.700632	248.321
medical_eq~p	186.5064	213.3442	4.57	0.000	19.81558	1755.42
RDservices	3.97611	3.424582	1.60	0.109	.7350677	21.50747
top25RD_04	.3687238	.2080946	-1.77	0.077	.1219869	1.114523

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
f19a_res_dev_a~4	Unmatched	691622.632	19531.0647	672091.567	65157.96	10.31
	ATT	663379.444	185479	477900.444	239183.689	2.00

Note: Sample S.E.

psmatch2: Treatment assignment	psmatch2: Common support Off suppo	On suppor	Total
Untreated	0	2,394	2,394
Treated	1	18	19
Total	1	2,412	2,413

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
f19_res_dev_4	Unmatched	.894736842	.164315353	.730421489	.08527729	8.57
	ATT	.888888889	.333333333	.555555556	.115620308	4.80

Note: Sample S.E.

psmatch2: Treatment assignment	psmatch2: Common support Off suppo	On suppor	Total
Untreated	0	2,410	2,410
Treated	1	18	19
Total	1	2,428	2,429

```
----------------------------------------------------------------------------------------
        Variable     Sample |    Treated     Controls   Difference         S.E.   T-stat
----------------------------+-----------------------------------------------------------
c5_num_employe~5  Unmatched | 9.05263158   3.69642857   5.35620301   2.76719271     1.94
                        ATT | 9.44444444   2.16666667   7.27777778   2.39597022     3.04
----------------------------+-----------------------------------------------------------
Note: Sample S.E.

 psmatch2: |   psmatch2: Common
 Treatment |        support
assignment | Off suppo  On suppor |     Total
-----------+----------------------+----------
 Untreated |         0      2,240 |     2,240
   Treated |         1         18 |        19
-----------+----------------------+----------
     Total |         1      2,258 |     2,259


----------------------------------------------------------------------------------------
        Variable     Sample |    Treated     Controls   Difference         S.E.   T-stat
----------------------------+-----------------------------------------------------------
did_innovation_5  Unmatched | .631578947   .162790698    .46878825   .085092633     5.51
                        ATT | .666666667   .333333333   .333333333   .152903988     2.18
----------------------------+-----------------------------------------------------------
Note: Sample S.E.

 psmatch2: |   psmatch2: Common
 Treatment |        support
assignment | Off suppo  On suppor |     Total
-----------+----------------------+----------
 Untreated |         0      3,655 |     3,655
   Treated |         1         18 |        19
-----------+----------------------+----------
     Total |         1      3,673 |     3,674


----------------------------------------------------------------------------------------
        Variable     Sample |    Treated     Controls   Difference         S.E.   T-stat
----------------------------+-----------------------------------------------------------
d5_a_lic_in_pa~5  Unmatched | .263157895   .015274034   .247883861   .029656548     8.36
                        ATT | .277777778   .055555556   .222222222   .100969452     2.20
----------------------------+-----------------------------------------------------------
Note: Sample S.E.

 psmatch2: |   psmatch2: Common
 Treatment |        support
assignment | Off suppo  On suppor |     Total
-----------+----------------------+----------
 Untreated |         0      2,226 |     2,226
   Treated |         1         18 |        19
-----------+----------------------+----------
     Total |         1      2,244 |     2,245
```

APPENDIX E

SELECTED PRIOR R&D SUBSIDY AND SBIR EVALUATION STUDIES

Author	Sample/Data	Method	Dependent Variable	Finding	Comparison with this Dissertation
Aerts and Schmidt (2008)	EU CIS III and IV	PSM and DID	R&D Expenditure R&D Intensity	On average, a Flemish company that receives a subsidy spends 0.837 million EUR (65%) more on R&D, compared to the situation where it would not have received the subsidy. German subsidized firms spend, on average, 3.232 million EUR more than non-subsidized firms. On average, the R&D intensity of German and Flemish funded companies is 64-100% higher than the R&D intensity of non-funded companies. Funded firms are significantly more R&D active than non-funded firms.	This dissertation found that, on average, SBIR recipient start-ups spent about $480,000 more in R&D than did observationally similar non-recipient start-ups. Both Aerts and Schmidt (2008) and this dissertation used PSM to establish the counterfactual outcomes of funded firms.
Berube and Mohnen (2009)	Canadian Survey of Innovation	PSM	Product Innovations Percent of Revenue due to Product Innovations	Positive Effect	This dissertation found that SBIR recipient start-ups are 33 percentage points more likely to introduce product and/or process innovations than observationally similar non-recipient start-ups. It supports the finding of Berube and Mohnen (2009) that R&D programs can have a positive effect on product innovations.
Czarnitzki and Licht (2006)	Mannheim Innovation Panel	PSM	R&D Expenditure Innovation Expenditure Patent Applications	Positive effect on R&D expenditure, innovation expenditure, and patent applications	This dissertation also found that SBIR recipient start-ups are more R&D active than observationally similar non-recipient start-ups. However, we did not find a significant effect of SBIR subsidy on patent size partly due to (a) a smaller sample size and by extension, larger

					standard errors and (b) a shorter time frame of the study. Czarnitzki and Licht (2006) used four waves (i.e. 1994, 1996, 1998, and 2000) of the Mannheim Innovation Panel.
Gonzalez and Pazo (2007)	Spanish Firm Survey	PSM	Total R&D effort Private R&D Effort	R& D subsidies have a positive and significant effect on total R&D effort. That is, firms add the amount of subsidies to their private budget, not substituting private R&D investment by public funds Public funds do not significantly stimulate private expenditures	This dissertation also found that public R&D subsidy from SBIR did not substitute for private R&D investment. Specifically, SBIR recipient start-ups spent about $110,000 on top of the R&D subsidy received from SBIR. Like Gonzalez and Pazo (2007), we also found that SBIR subsidy did not stimulate private R&D expenditure. In the sample, a dollar of SBIR subsidy reduced private R&D expenditure by $0.16. Both Gonzalez and Pazo (2007) and this dissertation used PSM to establish the counterfactual outcomes of firms that received R&D subsidy.
Hussinger (2008)	EU CIS	Heckman's Selection Model	Net R&D Expenditure (i.e. Total R&D minus government R&D grant) New Product Sales	R&D subsidy has a positive effect on private R&D expenditure Public R&D subsidies stimulate additional private R&D investment. A multiplier effect in the sense that EUR1 public R&D funding generates more than EUR1 private R&D expenses. There is a positive effect of publicly stimulated R&D investment on new product sales	Unlike Hussinger (2008), this dissertation did not find a positive effect on private R&D expenditure. Hussinger (2008) found a multiplier effect of public R&D subsidy on private R&D investment of German manufacturing firms while this dissertation found a crowding-out effect of the SBIR subsidy. While this dissertation found a significant positive effect of SBIR subsidy on the introduction of product and process innovations, we did not observe any significant program effect on sales 1-2 years after the subsidy. The difference in results could be due

					to different (a) mechanisms in selecting German manufacturing firms and U.S. small firms that receive R&D subsidy, (b) design of the R&D subsidy programs between the two countries, and (c) study time frames. Hussinger (2008) used a 1992-2000 panel data of German manufacturing firms.
Koga (2005)	Panel Data of Japanese high-technology start-ups	Fixed Effects Panel Data Analysis	Company-funded R&D	The evidence is consistent with the complement hypothesis, i.e., that publicly-funded R&D does promote private R&D.	This dissertation found that $1 of SBIR subsidy decreased private R&D by $0.16. The difference in results is due to different designs of the two R&D programs, i.e., the SBIR program in the US and the SRDCT program in Japan. SRDCT only covers up to 50% of total R&D expenditures of recipient small firms while SBIR does not require a matching R&D expenditure counterpart from recipient small firms.
Lee and Cin (2010)	Panel Data on Korean Manufacturing Firms	2SLS and 2-step Tobit	Private R&D expenditure	Government R&D subsidies in Korea induce additional company-funded R&D activities, rather than displace private R&D investment of the SMEs.	This dissertation found that the SBIR subsidy crowded out private R&D investment. The difference in empirical results could be attributed to design differences between the Korean R&D subsidy program for new technology development and technology transfer and the U.S. SBIR program.
Lerner (1999)	541 SBIR recipients + 891 matched firms	OLS Regression after matching on firm size and industry classification and firm size and geographical location	Sales Employment Attracting VC funding	Positive effect of SBIR on sales and employment but only in areas with substantive VC activity SBIR recipients are 1.4 percentage points ($p<0.01$) more likely to receive VC funding than matched non-recipient small firms	This dissertation found that SBIR recipients are 14 percentage points more likely to generate sales and 11 percentage points more likely to generate international sales 1-2 years after receiving the subsidy than their observationally similar non-recipient counterparts, but these outcome advantages are not statistically significant due to smaller sample size and thus, larger

					variances of the treatment effect estimates. Like Lerner (1999), we found a positive effect of SBIR on employment size. Unlike Lerner (1999), we did not find a significant positive certification effect of SBIR on attracting external, regardless of the source of external capital. However, we found a different form of certification effect of the program: SBIR recipients are more likely to outsource external knowledge assets. This finding implies that the SBIR award certifies the quality of the recipient-company improving its ability to persuade other innovators to license out their patents to the SBIR grantee. The difference in results could be due to differences in matching method and sample. This dissertation used statistical matching to balance 14 observable characteristics of recipient and non-recipient start-ups before the treatment effect of SBIR was estimated. Lerner (1999) matched only on firm size, industrial classification, and geographical location.
Ozcelik and Taymaz (2008)	Turkish Annual Survey of Manufacturing Industries	PSM	R&D intensity, Own R&D Intensity	Positive effect on both R&D intensity and own R&D intensity	This dissertation found that the SBIR subsidy did not have a positive effect on firm-financed R&D of small business start-ups. R&D subsidy program in Turkey provides grants that support only up to 50 percent of total R&D expenditure while SBIR does not have this requirement. The SBIR grant can fund up to 100 percent of total firm R&D.
Toole and Turvey (2009)	10,914 SBIR grantees 1983-1999	Probit regression	VC capital	Size of Phase 1 dollars positively affects follow-on VC investment Receiving Phase II positively affects follow-	This dissertation did not find a significant positive certification effect of SBIR on attracting external,

				on VC investment Number of Phase 1 and Phase 2 awards negatively affects follow-on VC investment	regardless of the source of external capital. What we found instead was a different form of certification effect: SBIR recipients are more likely to outsource external knowledge assets. This finding implies that the SBIR award certifies the quality of the recipient-company improving its ability to persuade other innovators to license out their patents to the SBIR grantee. Toole and Turvey (2009) used only SBIR grantees in their empirical analysis.
Wallsten (2000)	367 SBIR recipients; 90 rejected firms; 22 "eligible" firms that did not apply for SBIR funding. Final sample: 81	IV/3SLS	Firm-financed R&D Employment	One SBIR dollar is correlated with a reduction of $0.82 (p<0.01) in firm-financed R&D No effect on employment	This dissertation also found that the SBIR subsidy is associated with a reduction of privately financed R&D. The difference between the two studies is in the size of the crowding-out effect. Wallsten (2000) found that $1 of SBIR subsidy decreased firm-financed R&D by $0.82 while this dissertation found a smaller crowding-out effect of $0.16. The difference could be due to the sample used by the two studies. This dissertation's sample included only start-up firms while Wallsten (2000) drew a sample from the population of both young and established/older small firms. PSM estimates ATT while IV estimates LATE.

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